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BINARY STARS OF SHORT PERIOD.

By J. E. GORE, F.R.A.S., M.R.I.A.

THE Binary, or revolving double stars, are among the most interesting objects in the heavens. The number now known probably amounts to nearly 1,000. In most of these, however, the motion is very slow, and in only about 60 cases has the relative change of position since their discovery been sufficient to enable an orbit to be computed. In most cases the plane of the real orbit, or ellipse, described by the companion round the principal star, is inclined to the line of sight, and is therefore foreshortened into a more elongated ellipse. Indeed, there is only one case in which we see the *real* orbit of a binary star, viz. in the star μ Draconis, the orbit plane of which, according to Berberich, is at right angles to the line of sight. But the computed period —648 years—is very long; and the orbit, therefore, of somewhat doubtful accuracy.

The relation of the *apparent* ellipse, which we see, to the *real* ellipse, will be understood by the following illustration. Suppose a cylinder, or rod, of an elliptical, not circular section, to be cut across obliquely to its axis. Then this oblique section will represent the *real* orbit of a binary star, and the section at right angles to the axis the *apparent* orbit. The angle between these two sections will represent the inclination of the real orbit to the plane of projection or background of the sky. In the apparent orbit the primary star, which is supposed to be situated in one of the foci of the *real* ellipse, does not lie in the focus of the apparent ellipse; and from its position in this latter ellipse we can deduce mathematically the particular angle at which the oblique section must be made, to agree with the observed place of the primary star; and other details respecting the *real* ellipse.

The periods of revolution of the computed orbits vary in length from $11\frac{1}{2}$ to 1625 years. Of the 60 orbits which have been computed, there are about twenty-one with periods under 100 years, and these form the subject of the present paper. The orbits of many of these have been well determined, and some account of the most remarkable, with diagrams of the orbits, may prove of interest to the general reader.

1. δ *Equleii.*—This has the shortest period of any known binary star.* Over three complete revolutions have been described by the companion star round the primary since its discovery in 1852. The period found by the Russian computer Wrublewsky—11·478 years—is probably not far from the truth, but, as his orbit does not represent the measures very satisfactorily, I have not drawn the apparent orbit. It is, however, a very elongated ellipse, owing to the high inclination of the real orbit. Burnham found only "a slight elongation" in the star with the great 36-inch refractor of the Lick Observatory in July 1889. The distance between the components does not at any time exceed half a second of arc, so that it is beyond the reach of all but the largest telescopes.

2. ζ *Sagittarii.*—This bright southern star is also a close

* That is, of any binary which has been seen with a telescope as double. Mr. Gore necessarily leaves out of account in this paper such binary systems as those of ζ *Ursæ Majoris* and *Algol*, the existence of which has only been proved by the indirect evidence of the spectroscope and photometer. But our knowledge with respect to the orbits of some of these binary systems of very short period is rapidly increasing, and continued observations will probably give us data from which all the elements of their orbits may be determined, even though we may never be able to see them as double with the telescope. In view of the evidence we already possess, one may assert with confidence that masses of matter, large enough to be seen as stars, revolve about one another in periods ranging from less than eight hours upwards.—A. C. RANYARD.

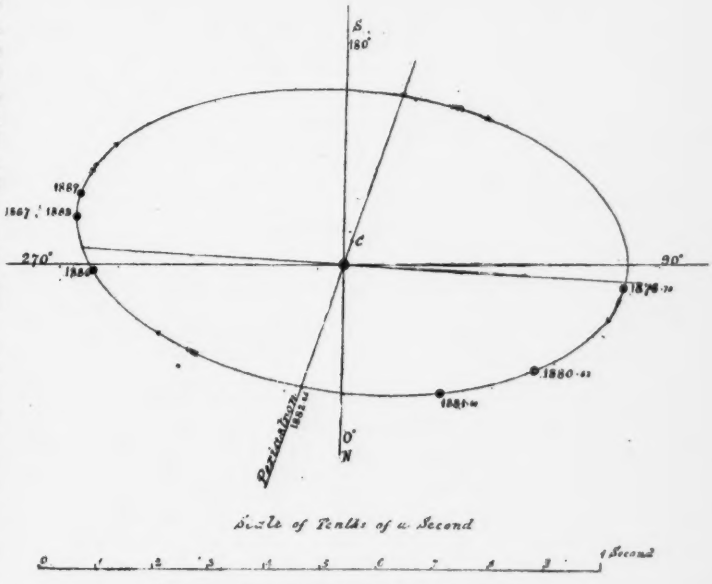

FIG. 1.—APPARENT ORBIT OF ζ SAGITTARII.

binary. An orbit computed by the present writer in 1886 makes the period 18·69 years, but recent measures seem to show that the true period is somewhat longer —perhaps about twenty years. Fig. 1 represents the apparent orbit, and shows the position of the companion at different times from 1878 to 1889.

3. 42 *Comæ Berenices.*—The period of this star has been accurately computed, and is about $25\frac{3}{4}$ years. The orbit is remarkable from the fact that its plane passes through, or nearly through, the earth. The orbit is therefore projected into a straight line, as shown in the diagram, Fig. 2. I have also drawn the *real* orbit, as it would appear could we view it from a point at right angles to its plane. I find that the plane of the real orbit is at right angles to the general plane of the Milky Way.

4. *β Delphini.*—The close duplicity of this star was discovered by Burnham in 1873. The period is short, but still somewhat doubtful. Celoria finds 16·955 years, Doubiago 26·07, and the present writer 30·91 years. Celoria's orbit is perhaps the best.

5. *ζ Herculis.*—Three complete revolutions of this remarkable pair have been performed since its discovery by Sir William Herschel in 1782. Several orbits have been computed, but Dr. Doberck's, with a period of 34·411 years, is perhaps the best. From his elements I have drawn the apparent ellipse represented in Fig. 3. The companion is now near its maximum distance ($1\frac{1}{2}$ seconds) from the primary star, and is within the reach of telescopes of moderate size. The companion is, however, rather faint, being only $6\frac{1}{2}$ magnitude, while the principal star is of the 3rd magnitude. When at their nearest some observers have spoken of "an occultation" of one star by the other; but the diagram will show clearly that no *real* occultation ever takes place, the components never approaching within half a second of arc. An occultation of one component of a binary star by the other cannot take place except—as in the case of 42 Comæ—the plane of the orbit passes through the earth.

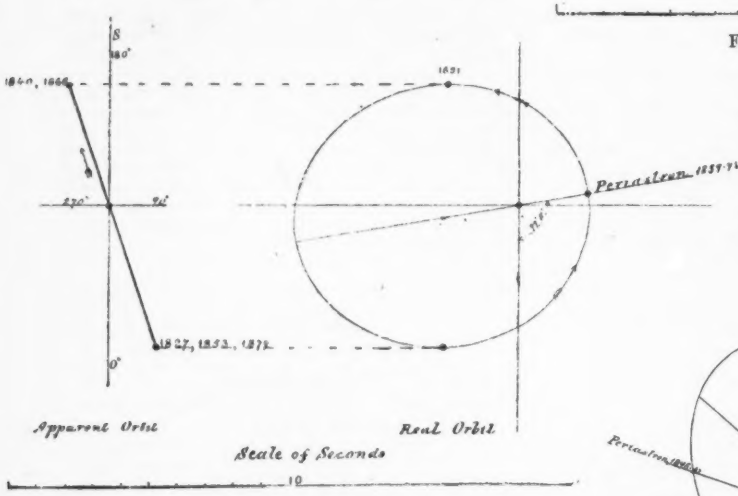

FIG. 2.—42 COMÆ BERENICES.

6. *η Coronæ Borealis.*—Some forty years ago it seemed uncertain whether the period of this interesting binary star was 43 or 66 years. But now that a complete revolution has been described, the question has been decided in favour of the shorter period. About a dozen orbits have been computed, and of these those by Dr. Doberck and Dr. Dunér, giving a period of about $41\frac{1}{2}$ years seem to be the best. The components are of nearly equal magnitude (6 and $6\frac{1}{2}$), but at their present distance

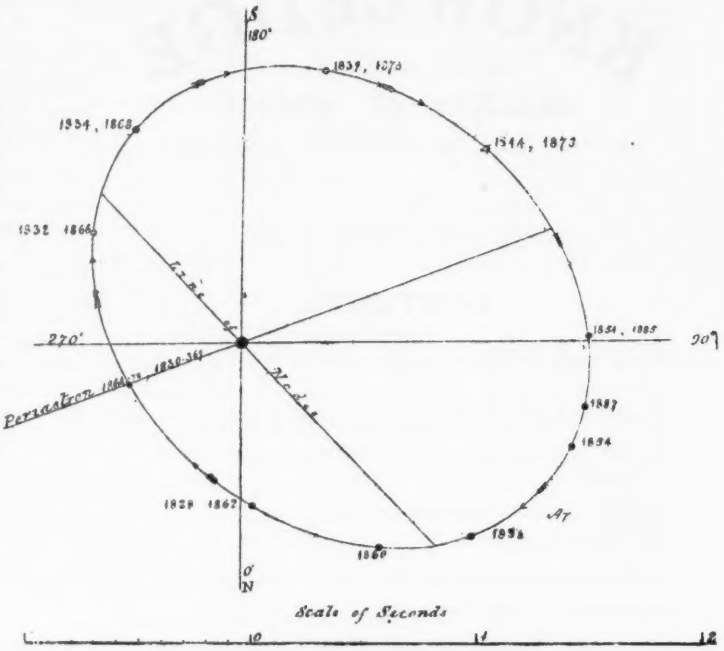

FIG. 3.—APPARENT ORBIT OF ζ HERCULIS.

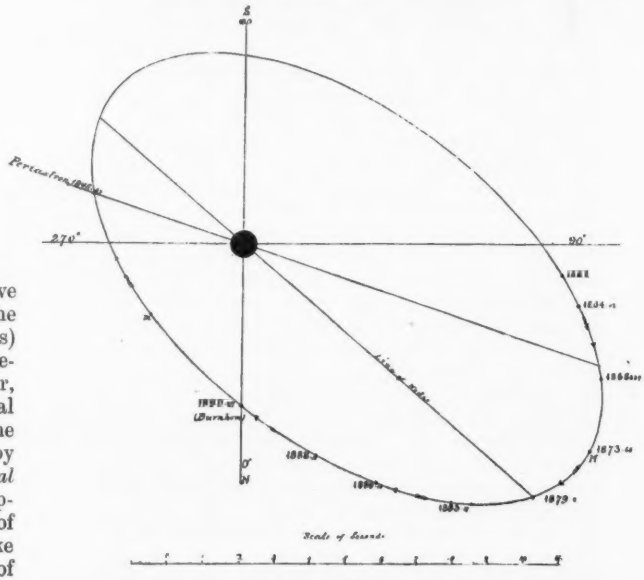

FIG. 4.—SIRIUS. APPARENT ORBIT.

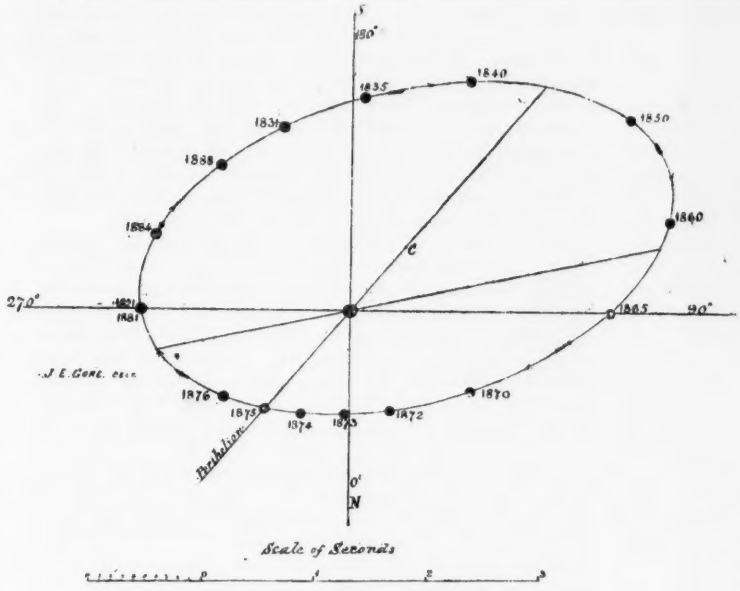

Fig. 5.—Apparent Orbit of ξ Ursæ Majoris

(about $\frac{3}{4}$ second) the star is not within the reach of small telescopes.

7. *Struve* 2173.—In this pair a complete revolution has been described since its discovery by Struve in 1829. The components are nearly equal, about 6th magnitude; but the present distance is less than one second of arc. Dunér's orbit, with a period of about $45\frac{1}{2}$ years, seems to be a good one, as it represents recent measures satisfactorily. The *real* orbit is nearly circular, but owing to its high inclination (nearly 81°) the *apparent* orbit is a very elongated ellipse.

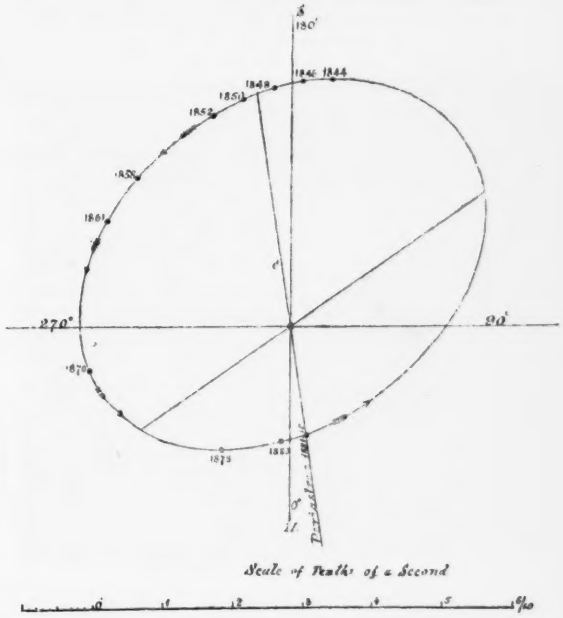

Fig. 6.—Apparent Orbit of O. Struve 234.

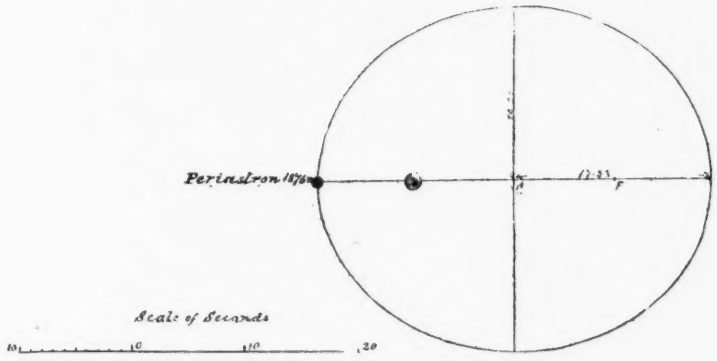

Fig. 7.—A Centauri. Real Orbit.

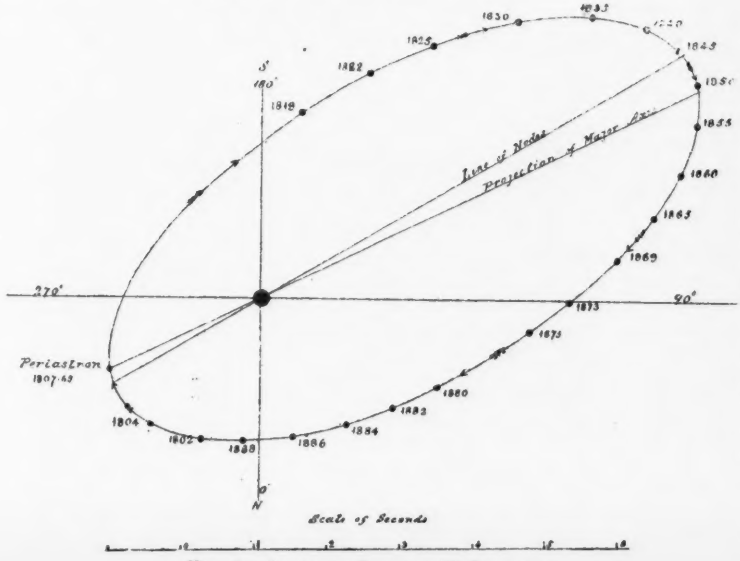

Fig. 8.—Apparent Orbit of 70 Ophiuchi.

8. *Sirius.*—The faint companion to this brilliant star was discovered by Alvan Clark in 1862. Some irregularities in the proper motion of Sirius led Bessel, in 1844, to suggest the existence of a disturbing body, and Peters, in 1851, calculated an hypothetical orbit for the supposed companion. He found a period of about fifty years, with an ellipse of large eccentricity. An investigation was also made by Safford in 1861, and when Alvan Clark discovered the now well-known companion its position was found to agree fairly well with that of Safford's hypothetical body. The companion, which is of about the tenth magnitude, has been regularly observed since, and several orbits have been computed giving periods of about fifty years. Recent measures, however, show, I think, that this period is somewhat too small. My period of 58·47 years (computed in 1889) represents all the measures fairly well. Observing with the 36-inch refractor of the Lick Observatory Mr. Burnham says:—"The companion to Sirius is a very easy

object, under proper conditions, and is not likely to ever get beyond the reach of the large refractor."

Assuming a parallax of 0·40″ (which is about the average of recent measures) my elements give the sum of the masses of Sirius and its companion equal to 2·886 times the sun's mass, with a mean distance between them of 21·45 times the sun's distance from the earth. I find that the plane of the orbit lies at right angles to the general plane of the Milky Way. The diagram, Fig. 4, shows the *apparent* orbit as found by me. The letter *m* marks the point of minimum distance (and M that of maximum distance). This diagram shows clearly that the minimum distance between the components of a binary star does not always occur at the periastron, as some have supposed. C is the centre of the *apparent* ellipse.

9. γ *Coronæ Australis.*—For this remarkable southern binary star several orbits have been computed, with periods ranging from 55 years to nearly 101, but none altogether satisfactory. An orbit by the present writer gives 81·78 years, and one recently computed by Mr. Powell 93·338 years.

10. ζ *Cancri.*—A well-known triple star, the close pair revolving in a period of about sixty years. Nearly two revolutions have now been completed since its discovery

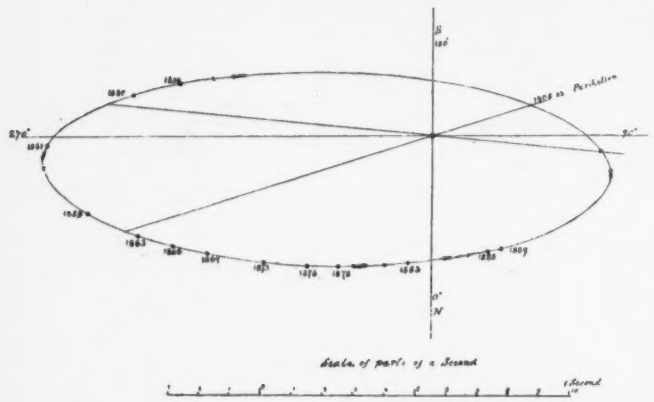

Fig. 9.—Apparent Orbit of Struve 228.

by Sir W. Herschel in 1781. All three stars form probably a connected system, but the motion of the third star round the binary pair is very slow and irregular. Prof. Seeliger has recently investigated the motion of this interesting system, and has come to the conclusion that to make the observations agree with theory it is necessary to assume that the third star is in reality a very close double, the components of which revolve round their centre of gravity in about 17·6 years, and both round the common centre of gravity of the components of the close pair. The supposed duplicity of the third star has not, however, yet been detected with the telescope. Burnham, in 1889, using a power of 1,500 failed to see any other component.

11. ξ *Ursæ Majoris.*—This very interesting binary was the first pair for which an orbit was computed—by Savary in 1830. More than a complete revolution has now been performed since its discovery by Sir W. Herschel in 1780. The period has, therefore, been well determined, and seems to be about sixty years. Fig. 5 represents the apparent ellipse, which I have drawn from Dr. Dunér's orbit. From this it will be seen that although the components are not at present near their maximum distance apart, they are yet within the range of small telescopes, the distance being about $1\frac{3}{4}$ seconds, and the magnitudes of the components not very unequal, about four and five.

12. *O. Struve*, 234.—This is a very close double star for which I computed an orbit in 1886, and found a period of 63·45 years. Owing to the discordancy of the measures, however, this orbit will probably require revision when further measures are available. Fig. 6 represents the apparent orbit I found, and shows the position of the companion from 1844 to 1880. It will be seen from the diagram that the maximum distance between the components is less than half a second of arc, so that the pair is beyond the reach of all but the largest telescopes.

13. *α Centauri.*—This bright southern star, the nearest of all the fixed stars to the earth, is also a remarkable binary. The magnitudes of the components are, according to Gould, 1 and $3\frac{1}{2}$, but as a single star it is brighter than an average star of the 1st magnitude, and about equal in brightness to Arcturus. Although it has been carefully measured for many years the period is still somewhat uncertain. While Downing and Elkin make it 76 or 77 years, Powell maintains that a longer period of about 87 years is more probable. Fig. 7 represents the *real* ellipse as drawn from Downing's correction of Elkin's orbit. Assuming a period of 77 years, and a parallax of 0·75″, I find the sum of the masses of the components 2·14 times the mass of the sun, and the mean distance between them 23·333 times the sun's distance from the earth, or somewhat greater than the distance between the sun and Uranus.

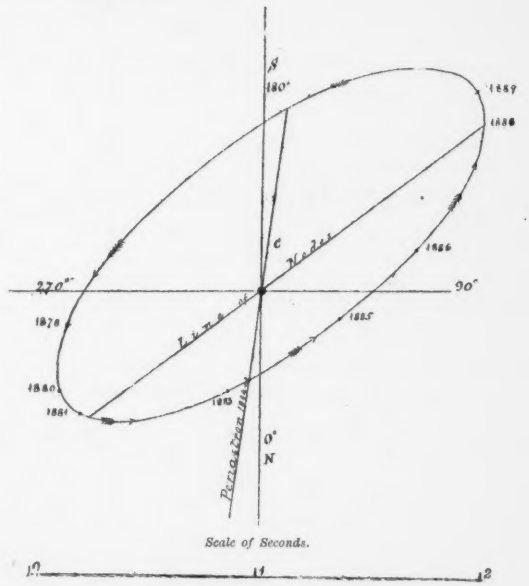

Fig. 10.—Apparent Orbit of ξ Scorpii.

14. 70 *Ophiuchi.*—A splendid double star; magnitudes about 4 and 6. More than a complete revolution has now been described by the companion since its discovery, by Sir W. Herschel, in 1779. Numerous orbits have been computed, with periods ranging from $73\frac{3}{4}$ to $94\frac{1}{3}$ years, but none altogether satisfactory. I have recently computed an orbit, and find a period of 87·84 years. This orbit represents all the measures satisfactorily, and cannot, I think, be far from the truth. The diagram, Fig. 8, represents the apparent orbit I find, and shows the position of the companion in various years, from 1802 to 1888. The distance between the components is at present slightly increasing as the companion approaches the periastron; after which it will diminish for some years, but

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From Photographs taken at the Lick Observatory by Prof. E. E. BARNARD.

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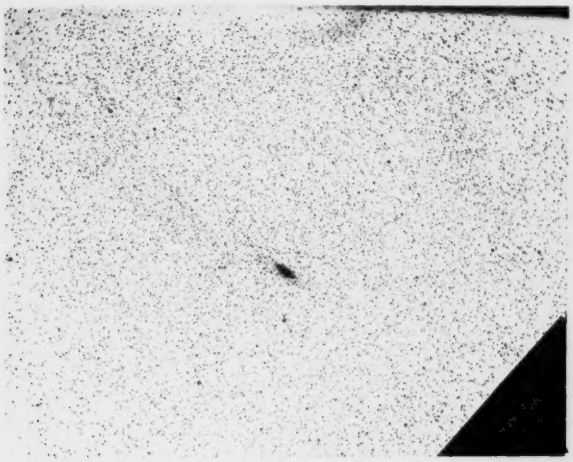
1. REGION ROUND THE GREAT NEBULA OF ANDROMEDA.
Scale—1 inch = 5° 18′.



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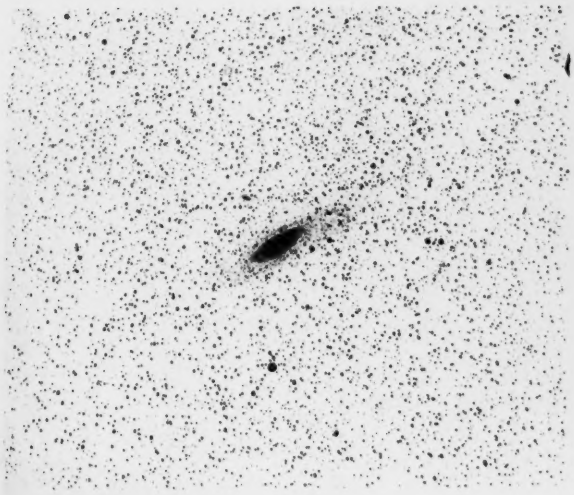
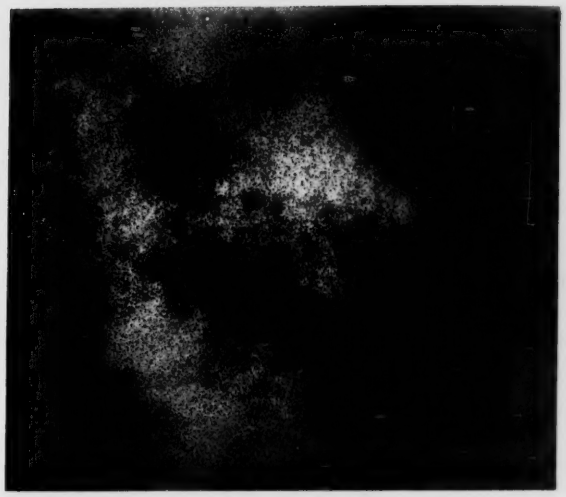
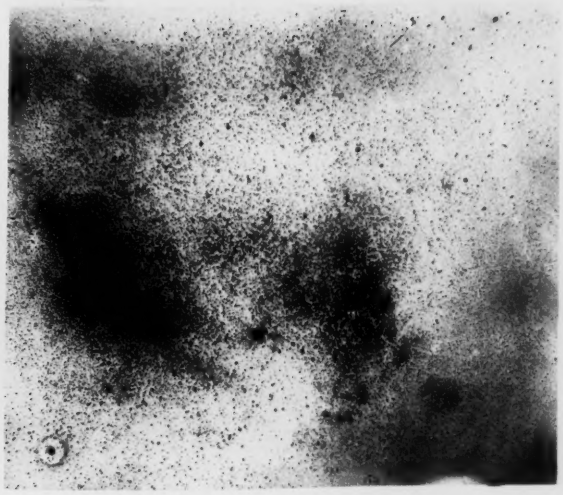
2. REGION OF MILKY WAY SOUTH OF THE TRIFID NEBULA.
The black patch at the top is the over-exposed image of Jupiter. Scale—1 inch = 4° 36′.







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3. MILKY WAY IN SAGITTARIUS.
Photograph taken 25th July, 1889. Region round 24 Messier
18h. 10m. $\delta = -18° 28'$. Scale—1 inch = 1° 48′.

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4. MILKY WAY IN AQUILA.
Photograph taken 2nd Aug., 1889. Region round 11 Messier
18h. 42m. $\delta = -6° 27'$. Scale—1 inch = 2° 30′.

Direct Photo Eng. Co., Limited, 9, Barnsbury Park, N.

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will never be less than $1\frac{1}{2}$ seconds, so that this fine pair will always remain within the range of moderate sized telescopes. My elements, combined with Krüger's parallax of 0·162″, give the sum of the masses = 2·777 times the mass of the sun, and the mean distance between the components 27·777 times the sun's distance from the earth. I find that the plane of the orbit is at right angles to the plane of the Milky Way.

15. *Struve* 228.—I have recently computed an orbit for this pair which represents all the measures from 1829 to 1889, fairly well for so close a star. Fig. 9 represents the *apparent* ellipse I have found. According to this orbit the distance between the components, which is at present about 0·4″, will gradually increase during the next few years up to 0·55″, then diminish again as the companion

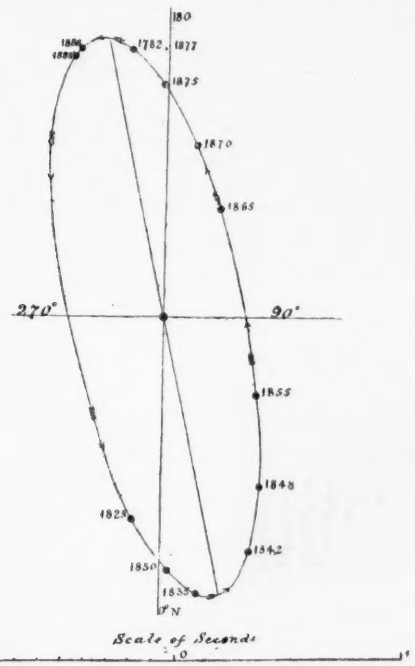

Fig. 11.—Apparent Orbit of 85 Pegasi.

approaches the periastron. The minimum distance will not be reached till the position angle is nearly 180° (after the periastron passage), when the components will probably be separated by less than 0·2″.

16. γ *Coronæ Borealis.*—For this close and difficult double star Dr. Doberck finds a period of $95\frac{1}{2}$ years. As in the case of 42 Comæ, the plane of the orbit nearly passes through the earth, and the apparent orbit is nearly a straight line. For the past ten or eleven years this star has been "single," even with the largest telescopes. I find that the plane of the orbit is at right angles to the plane of the Milky Way. An orbit has recently been computed by Celoria with a period of 85·276 years.

17. ξ *Scorpii.*—A remarkable triple star; magnitudes about $4\frac{1}{2}$, 5, and $7\frac{1}{2}$. The companion of the close pair has described a complete revolution since its discovery by Sir W. Herschel in 1780. Dr. Doberck finds a period of 95·9 years. The real orbit is nearly circular, but owing to its high inclination—about 70°—the *apparent* orbit is a very elongated ellipse, as shown in Fig. 10. All three stars have a common proper motion, and probably form one system, but the motion of the third star is very slow, and its period of revolution must be several hundred years.

18. 85 *Pegasi.*—A well-known wide double star; magnitudes about 6 and 9, and present distance about 22″. In 1878 Burnham discovered the brighter star to be a close and difficult double, and since that year the companion, which is about the 11th magnitude, has described no less than 220° of its apparent orbit. Mr. Schaeberle, of the Lick Observatory, has recently computed an orbit, and finds a period of 22·3 years, with a high inclination, which makes the apparent orbit, shown in Fig. 11, a rather elongated ellipse. Assuming a parallax of 0·054″ found by Brünnow, I find the mass of the system 11·297 times the sun's mass, with a mean distance of 17·777 times the sun's distance from the earth, or a little less than the distance of Uranus from the sun. As, however, the assigned parallax is so small, its accuracy is somewhat doubtful. A parallax of 0·054″ would imply a distance of 3,819,722 times the sun's distance from the earth, or about 60 years' journey for light. From the recorded measures of the distant companion I have computed the proper motion of the binary pair, and find it to be 1·221″ in the direction of position angle 141·25°. This proper motion combined with the above parallax would imply a real motion of about 66 miles a second.

HOUSE-FLIES AND BLUEBOTTLES.—VI.

By E. A. Butler.

FLIES are subject to the persecutions of animal as well as vegetable parasites, some of which attack them externally and some internally. The chief external one is a reddish kind of mite (Fig. 16). These creatures may be found, principally during the summer months, on different parts of the bodies of the flies, especially on the under surface; here, in the neighbourhood of the attachments of the legs, and at the junctions of the body segments, there are plenty of places—the joints of the armour, as it were—where the skin is thin enough for the mites to pierce with their snouts. They show a good deal of enterprise in endeavouring to secure the best places—no easy matter when there are a good number of them—and when one has plunged its proboscis into the flesh, it adheres most tenaciously, and its body may be lifted up and pushed from side to side without causing it to relax its hold. The flies, though so particular in removing from their persons the slightest trace of inanimate foreign matter, by sweeping and scraping themselves with their legs, yet submit patiently to the presence of their living burdens, even when they get into places whence they could, one would imagine, easily be removed. For example, a *Musca domestica* that has just at the moment of writing alighted on the window pane, flew about unconcernedly with a large mite clinging to its face in such a way as one might suppose would have seriously interfered with the use of both eyes and antennæ. It did not seem,

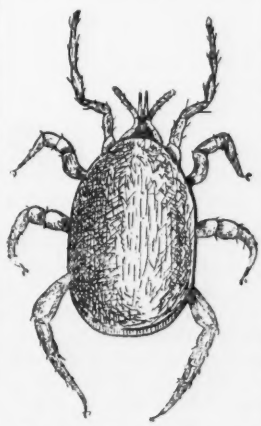

Fig. 16.—Mite Parasite on Fly. Taken from body of *Pollenia rudis*

however, in the slightest degree incommoded. Another external parasite sometimes, but not so commonly, found, is an animal belonging to a group closely allied to the mites, viz. the book-scorpions or Chelifers. It is a little reddish creature with a pair of great pincers in front like a scorpion, but differing in that the body does not taper away into a tail, but ends abruptly. Amongst the internal parasites are various kinds of small hymenopterous insects, allied to the ichneumon flies; and an instance is recorded of an exceedingly fine and hair-like nematoid worm, of the enormous length of three inches, having been taken from the abdomen of a house-fly. M. Fourment, who records the fact, states that notwithstanding that the parasite had caused a considerable enlargement of the body of its host, the latter did not seem in any way inconvenienced in its flight.

We have now enumerated eight species of *Muscida* which are more or less intimately associated with man, and which, either by reason of some peculiarity in their economy, or simply in consequence of their numerical abundance, often become a source of trouble and annoyance in the premises we occupy, damaging our food or other property, attacking our persons, or worrying and harassing our nervous susceptibilities. Some interesting questions arise in connection with this undesirable intimacy of relation, but many more observations will be needed before any very satisfactory answers can be given to them. It is not easy to understand, for instance, why these particular species of flies, rather than any others, have elected to attach themselves to man, and to follow his fortunes, as some of them have done, all over the world. It is not that they are so different from other flies that one would necessarily expect them to behave in an exceptional way; neither in structure nor even in habits, except in this one particular, is there anything which will broadly distinguish them from allied species which do not trouble us. There is absolutely nothing that would enable a person ignorant of the species to separate, in a given assortment of flies, those that are household pests from those that are not. We get one from one group, another from another, and so on, but they do not form a compact and isolated company. Their association with man, it is true, is not so complete as that of several other insects, such as the cockroach, the clothes moth, and the bed-bug, which spend their whole lives under the shelter of our houses, and propagate themselves generation after generation without ever troubling themselves about the outside world. As already mentioned, it is only in the last stage of their life that, as a rule, we are annoyed by these flies; but perhaps this limitation may be regarded as making the association all the more remarkable. That as the perfect stage is reached in each succeeding generation, the instinct to betake itself to the abodes of men should regularly recur to an insect born and bred in the open air, is, it would seem, more remarkable than that the association should be a continuous and permanent one. As the nature of the food on which they are reared necessitates, as a rule, that they should pass through their earlier stages exposed, it is rather curious that the perfect insects should not confine themselves to similar localities, but should also enter our dwellings, and often in such surprising numbers.

Nor is it, again, that they are so much more abundant than all other species, and that, therefore, mere excess of numbers causes them to be the species represented indoors; that, in other words, we simply get the overflow from outside. Of course they are abundant—this is implied in their being pests—but there are other species equally so, of which it is the rarest occurrence to find a specimen in the house. Take, for example, the case of *Sarcophaga carnaria*, the flesh fly, which has been several times referred to already. This is an insect of most extraordinary fecundity; it is said that as many as 20,000 eggs have been found in the ovaries of a single female, and, in consequence, it is an extremely common fly; but though its habits are similar to those of the bluebottle, and it swarms round human dwellings, it is very seldom seen indoors. The facts of its distribution seem to show that it is far less dependent on man, and far more inclined to ignore his movements, than our household pests. It is an extraordinarily hardy insect, and shows wonderful powers of adaptation to circumstances. Even in the matter of food, which is often such a critical point with a larval insect, it can stand some degree of variation, feeding not merely on meat, either fresh or putrid, and wounds and ulcers on men and other animals, but even on decaying vegetable matters and dung as well. Even if half-starved, it will still undergo its metamorphoses, though, of course, the perfect insects will be dwarfed. Like several others, it can even withstand the action of the digestive fluids of the stomach and intestine of living vertebrate animals. Bernard introduced it artificially into the stomach of a dog, but it passed along the intestine and was voided in the usual way alive; Portchinski's similar experiment with a frog had the same result. In the case of a little song-bird, however, the larva was dead when voided, but still undigested. That so common and

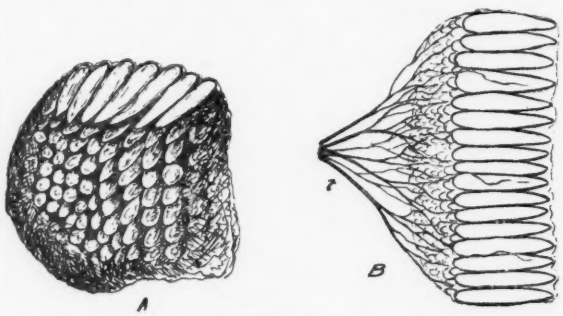

Fig. 17.

(A) Portion of Left Ovary of Bluebottle, containing about 80 Eggs.

(B) Side View of Portion of Right Ovary, showing Distribution of Tracheæ, or Air-Tubes (*t*), to the Organ.

so hardy a European fly should be one of the most likely to follow man's lead and migrate with him to other parts of the world would be only what was to have been expected, and yet, though the five flies mentioned at the commencement of these papers, together with *Cyrtoneura stabulans*, are as common in the United States as in Europe, though not indigenous there, *S. carnaria* has, according to Osten-Sacken, not yet been introduced into America, so that four centuries of European communication with the new world have not sufficed to import this abundant but independent species.

The curious observations of Portchinski have an important bearing on the subject, though perhaps they will hardly justify the conclusions he has drawn from them. He finds that carrion-feeding flies are, as a group, enormously prolific, while dung-feeding species are much less so; for example, *Calliphora*, a carrion-feeder, lays from 300 to 600 eggs, while *Musca domestica*, a dung-feeder, lays only about 120. (Fig. 17 shows the method of distribution of the eggs in the ovaries; they lie side by side in a compact mass.) These differences, he argues,

are connected with the different conditions, as regards the struggle for existence, under which the contrasted species live. There are, according to him, comparatively few species of carrion-feeding flies in our regions, so that they have it all to themselves, with little competition, the numerous beetles that have similar habits being said to feed quite as much on the fly larvæ themselves as on the carrion. Their action must, from the nature of the case, be rapid, and the supply of food he regards as plentiful. All these conditions favour multiplication, and have contributed towards producing the extraordinary fecundity for which the insects are noted. There is one other point which lessens the competition, viz. that the different species to some extent succeed one another during the summer in point of time (*i.e.* in the larval form), instead of being contemporaneous. This was proved in the following way: he laid out of doors the dead bodies of small vertebrates, such as rats, birds, &c.; the flies soon laid eggs upon them, and these were then collected and the maggots bred to maturity, whereby the relative numerical abundance of the species was ascertained. One of the chief results thus established was that *Cynomyia mortuorum*, an out-door flesh-fly, was abundant in spring, but *Calliphora vomitoria*, a bluebottle, did not begin to appear till about the beginning of June, when the former species had begun to decline. Later on, the proportions were reversed, the bluebottle being in excess, and the flesh-fly scarce. Thus everything favours the fecundity of the carrion-feeders.

But with the dung-feeders the case is different. Here there is much competition, there being large numbers of dung-feeding flies, as well as beetles, which latter do not feed on the former. The supply of food, too, Portchinski regards, curiously enough, as less abundant. These are circumstances which place fecundity at a disadvantage, and hence have sprung the more moderate powers of multiplication possessed by the dung-feeding flies. But here, again, a very curious circumstance has arisen, which gives an extra advantage to the less prolific species. There is a fly which is structurally very like *M. domestica*, differing chiefly in the brighter colour of its abdomen (Fig. 18), which is of some tint of yellowish or brown. Its name is *Musca corvina*, and it frequently hibernates in houses, and may therefore be reckoned amongst the household species. Notwithstanding the close resemblance between these two species in their perfect condition, as well as in that of the full-grown larvæ, there is a most extraordinary difference in the circumstances of their development. While the house-fly lays 120 eggs, *M. corvina* lays only 24, but they are much larger, and hence the larval life is able to be shortened; this is done at the expense of one of the two transformations. *M. domestica*, as before mentioned, passes through three stages in its larval life, while *M. corvina* has only two, the second of the three being, in its case, omitted altogether. This enables it to come to maturity sooner than its relative, and hence gives it an advantage which counterbalances its low degree of fecundity. In some such way as this, Portchinski considers that more prolific flies have been weeded out by less prolific ones from amongst the dung-feeders, so that the majority are now of the less prolific type. But *M. domestica*, with a degree of fecundity which, though low as compared with the carrion-feeders, is yet high for a dung-feeder, is apparently an exception amongst the latter, and herein M. Portchinski finds the explanation of its close association with mankind, the bond of union being, in fact, in this particular species probably closer than in any other, for the house-fly is said to be rarely found far from human dwellings. According to the above theory, the house-fly has sought the protection and additional resources of man's society to aid it in its struggles with less prolific insects, which, by their shorter larval life, would otherwise have hurried it out of existence. Whatever may be thought of these speculations, and it would obviously not be difficult to raise objections to them, still the observations on which they are based have revealed some very curious facts which require to be accounted for in some way or other, and which invest with special interest the history of the relations between insects and man. Farther researches by the same investigator show that the developmental history of an insect may depend very much upon climate, the same kind of fly developing in a different way in northern and in southern latitudes.

Fig. 18.—Distribution of Colour on Abdomen of Musca Corvina. The clear parts are yellow, the shaded parts black.

However exclusive the tastes of their larvæ may be, some at least of these flies seem to be almost omnivorous in their perfect stage, and therefore, possibly, food is one attraction which allures them into our houses; but then again comes the question "Why these alone? Why do not the abundant supplies man's providence stores up become equally enticing to other closely allied forms, whose tastes and needs one would suppose to be similar?" Flies generally manage to find out the room in which the provisions happen to be placed, though the aspect of the apartment has certainly quite as much to do with the numbers that find their way thither as the mere presence of eatables; if the room be bright and sunny, the flies will swarm, while the same provisions in a dull and shady room will be almost ignored. And again, apart from the occasional intrusion of an unwelcome bluebottle making straight for the cold meat, the bringing in of meals does not usually produce any noticeable increase in the number of flies in the room; often many of those that are there seem supremely indifferent to the viands that may be displayed, and continue to amuse themselves by sporting about the windows, mirrors, picture-frames, or gas pendants. We must not forget, however, that what is not food to us may yet be so to them, and they appear to find in these various household objects some sort of nutriment, to judge from the industrious way in which, for example, they will travel over the painted window-frames, dabbing their proboscis down with as much persistence and energy as if they were making a most luxurious feast. What is it they get? Is it the varnish, or the oil with which the paint was mixed, or is it the thin film of miscellaneous matter—dirt we call it—which gradually accumulates on every exposed surface? On any of these suppositions, one would suppose that there would be at least as much to be obtained out of doors as inside, and probably a good deal more.

Such species as hibernate in the house, like *Pollenia rudis*, no doubt come in for the sake of shelter from winter's cold. These gradually accumulate, instead of suddenly coming in a swarm. An instance has been recorded of two other species of the same genus swarming in the same building, to the exclusion of others, for seven successive years; the suggested explanation was that there were certain conditions which facilitated the entrance of the flies, but rendered their exit difficult.

The Editor regrets that want of space has prevented the insertion of any Notices of Books this month.

THE BREAD-FRUIT TREE AND THE NETTLE FAMILY.

By R. Camper Day, B.A.Oxon.

THE first description of the bread-fruit was given in 1688 by Dampier. He declared its flavour to be intermediate between those of bread and roast chestnuts. "The inside," he said, "is soft, tender, and white, like the crumb of a penny loaf. There is neither seed nor stone in the inside, but all of a pure substance like bread." About a hundred years later, Captain Cook described the plant more accurately. "The tree that bears the bread-fruit is about the size of a horse-chestnut; its leaves are near a foot and a half long, in shape oblong, resembling in almost every respect those of the fig-tree; its fruit is not unlike the Cantaloupe melon either in size or shape; it is enclosed in a thin skin, and its core is as large as a person's thumb; it is somewhat of the consistency of new bread, and as white as blanched almond; it divides into parts and they roast it before it is eaten; it has little or no taste." Everybody knows how much interest was aroused in this country by Captain Cook's praises of the bread-fruit, and how the *Bounty* was sent out for the purpose of transplanting a number of specimens to the West Indies, where it was supposed (wrongly, as the event proved) that the newly-discovered vegetable would supersede the banana as the staple food of the natives. The transplantation was successful enough, but it was soon found that the bread-fruit could not compete with the banana in rapidity of growth, and in the production of a maximum quantity of food with a minimum of labour. The bread-fruit, however, has undoubtedly better qualities as a food. It has, according to Mr. A. R. Wallace, "a slight and delicate but very characteristic flavour, which, like that of good bread and potatoes, one never gets tired of." In some kinds there are large seeds, which ripen; but in those commonly used for food (and the bread-fruit forms the chief food of the South Sea Islanders) the seeds are aborted, and the whole fruit can be eaten.

The general appearance of the bread-fruit when nearly ripe is well shown in one of the photographs which accompany this article. The larger of the two fruits is about eight inches long. The colour is a light green, changing to yellow at maturity.

Although the size of the fruit and the close resemblance between the edible portion and ordinary bread are the points that have appealed to the popular imagination, the bread-fruit tree is not without other remarkable characteristics, which are not so generally known. In the first place it is a member of the very interesting family to which our nettles belong. At first sight, nothing would seem more dissimilar than the bread-fruit, as shown in the photograph, and our common stinging nettle; not to mention other points of difference, the leaves of the former are large, smooth, and leathery, while those of the latter are small, and densely covered with down and stinging hairs. The chief resemblance is in the flowers. All the nettle tribe have separate male and female flowers; in some cases both are to be found on the same plant; in others, as with our commonest kind of nettle, an individual plant bears male or female flowers exclusively. On the bread-fruit tree the male flowers are clustered in large catkins, roughly resembling the heads of bulrushes; the female flowers are clustered upon small round balls, which afterwards expand and become the bread-fruit. Of the three kinds of nettles in Great Britain, the rarest has several peculiarities which remind one of the bread-fruit. It is monœcious, that is, both male and female flowers are found on the same specimen; and the female flowers are clustered together into little round balls which have earned for the species the distinctive name of "pill-bearing" (*pilulifera*).

The English representatives of the family are very few. Besides the three kinds of stinging nettles above mentioned, there are the common pellitory and the hop, and these exhaust the list, unless we include the elm, which is closely allied. But the tropical species are very numerous. Among these may be reckoned the hemp, which, although cultivated in England, is not a native; the mulberry; the banyan, remarkable for the adventitious roots thrown down from the boughs, by means of which its crown can be indefinitely extended; and the slow-growing caoutchouc plant, so much used for decorative purposes. Perhaps the two members of the family which, for different reasons, have acquired the greatest celebrity are the peepul and the upas. The former is the sacred tree of the Buddhists and Hindoos. The venerable specimen shown in our second photograph is the Sacred Bo Tree of Anhuradhpur, Ceylon. It is probably the oldest existing tree of which the date of planting is recorded. Taken as a cutting from the tree in which Buddha himself was supposed to have been cradled, it was planted in the year 288 B.C. It is now in a somewhat decrepit condition, as shown by the props supporting the branches; but no one is allowed to injure it, and only the leaves which fall naturally are distributed to pilgrims. The upas tree of Java, on the other hand, enjoyed for some time a notoriety of a most unenviable kind. About a hundred years ago a traveller from Java published in the *London Magazine* a very exaggerated account of the tree. He declared that the vapour given off by it was so poisonous as to destroy almost all living things within a radius of fifteen miles. Condemned malefactors, he said, were sent to fetch the poison, and not more than two in every twenty returned alive. The upas did not deserve the evil reputation which it acquired in consequence of this description. As a matter of fact, the tree grows in thick forests, and, although it secretes a deadly poison (strychnia), it does no harm to living things near it unless they actually rub against it; and even then it merely causes an irritation of the skin.

But the fact that the upas secretes poison is undoubted, and indeed the secretion of fluid matter, poisonous or otherwise, is characteristic of the whole nettle family. The stinging hairs of the common nettle, and the bitter gum-resin of the hop, as used in brewing, are well known to everybody. Three different kinds of poisonous material are extracted from hemp. They are known as bhang, ganjah, and churras, and all of them are smoked in India in much the same manner as opium. But there is another and more harmless kind of secretion universally characteristic of the nettle family. If you cut off the top of a common nettle a white milk-like liquid immediately begins to exude from the wounded end of the stem. It consists of a watery fluid, with exceedingly small granules suspended in it, which, as in the case of cow's milk, are the cause of its opaque appearance. When the fluid exudes and comes into contact with the air the corpuscles have a tendency to stick together and harden into a gummy substance. In every member of the nettle family this fluid is to be found. The cow-tree, a native of Venezuela, yields from incisions in the trunk a particularly copious white juice, resembling milk in taste, which is used as an article of food; and nearly all the tropical species of nettles yield it in the form of lac, resin, or india-rubber.

The question, then, arises, What is this peculiar liquid, and what useful office does it perform in the economy of

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— FICUS RELIGIOSA.
THE SACRED BO-TREE AT ANURADHAPURA.
UNDER WHICH SIDDHARTHA OBTAINED
BUDDHAHOOD, SITE FIXED IN INDIA BEING
THE IDENTICAL TREE UNDER WHICH
GAUTAMA IS SAID TO HAVE ATTAINED
THE BUDDHASHIP. THE ORIGINAL BRANCH
WAS PLANTED BY KING DEVANAMPIYA TISSA, B.C. 288

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plant life? It is certainly not the sap. The sap consists of water sucked up by the roots and the products of assimilation drawn from the leaves. The sap is entirely liquid, and contains no solid bodies in suspension. It does not circulate through the plant in tubes; for the substance of a plant is built up of closed cells and vessels. The transference of the sap from cell to cell can only take place by what is called diffusion, that is, by penetrating through the partitions between the cells; and, moreover, it is probable that the greater part of the water which ascends from the roots to the leaves finds its passage not by this slow diffusion but by permeating continuously the substance of the walls which enclose the cells. It is clear, therefore, that the sap cannot convey solid particles, and consequently the juice which exudes from the wounded stem of a nettle cannot be the sap, because, as already stated, it contains minute granules in suspension.

This juice is known to botanists as the Latex. Unlike the sap, it is contained in continuous tubes or veins, which ramify in innumerable quantities throughout the entire substance of the plant. If it were possible by any means to destroy all the other tissues of such a plant as a large *Euphorbia* (for the nettle family is by no means the only family in which these latex tubes are found), then, as Sachs has pointed out, "the entire form of the plant would still be preserved as a mass of very fine threads of various thickness, representing the ramifications of the original latex-cells; just as the injected vascular system of a vertebrate animal after the removal of all other tissue allows the whole organisation of the body to be recognised." From the fact that the latex-cells run in such large numbers through every portion of the tissues it is evident that their contents must play a very important part in the physiology of the plant. The exact nature of that part has not been completely made out, but this much may be said, that it is analogous in some respects to the part played by blood-vessels and veins in animals. In other words, the latex-tubes contain substances of two kinds, those which in combination with the liquids and gases absorbed by the plant are utilised in its growth, and those which are not so utilised but remain in the latex-tubes as excretions or secretions.

It would be incorrect to suppose, however, from the analogy of the blood-vessels in an animal, that there is anything in the nature of a circulation of the latex in the latex-tubes. When an animal is wounded the blood immediately begins to flow, in consequence of the pumping action of the heart. The fact that when a latex-bearing plant is wounded the latex immediately exudes, might seem at first sight to be evidence of a circulation of the latex. If the bore of an extremely fine glass tube be filled with liquid, and the tube be then broken in the middle, there will be no discharge of the liquid from either of the broken ends. Assuming, therefore, that the latex does not circulate we must look for some special reason to explain the fact that it flows so readily from the smallest incision.

The cause we seek is to be found in the phenomenon known as "turgescence," and the manner of its action is explained by a simple experiment given in Sachs' admirable work on the physiology of plants. If we cut a fresh and flourishing leaf-stalk from a rhubarb plant, we have a fairly rigid and elastic staff which does not bend by its own weight when we hold it horizontally by one end. If a strip of the skin be peeled neatly off from one end to the other, and we then try to fit the piece of skin back into into its place, we find that it is too short. We may infer, therefore, that before it was peeled off it was in a state of tension, and that as soon as it was removed it contracted. If instead of removing merely a strip we peel off all the skin, we find not only that the skin becomes shorter, but that the remainder of the stem becomes considerably longer. If the stem was originally a foot long, it may now be as much as thirteen inches. The skin is limp and flexible, and so also, in a less degree, is the soft interior of the stem; the soft interior has still a certain amount of stiffness, for the substance of the cell-walls and vessels contribute in some measure to the rigidity of the stalk, but neither the skin nor the stripped interior can be held horizontally without bending. In fact, the stiffness of the uninjured stalk is mainly due to the mutual tension and pressure between the skin and the mass of soft tissue enclosed in it. It is evident, therefore, that the substance of a plant is under strong hydrostatic pressure, to which much of its rigidity and elasticity is attributable. A single cell is compared by Sachs to "a thin-walled caoutchouc balloon which, when empty, is a limp, wrinkled sac, but which may be converted into a firm, elastic sphere by being strongly inflated with air." "If we suppose," he says, "some hundreds of thousands of small caoutchouc balloons thus inflated with air, and all contained together in an extensible caoutchouc vesicle, the latter with its contents would form a rigid bar, like the stem of a plant. If we suppose the small caoutchouc balloons not inflated with air but filled tense with water, the same effect results; and it is somewhat in this manner that we have to imagine the rigidity of a stem produced by the turgescence of the cells." From this it is sufficiently clear that the expulsion of the latex from a cut surface is caused by the high pressure in the substance of the plant, and we have thus a complete solution of one of the many problems suggested by the latex-tubes.

Letters.

Reprinted from the "Times" of 5th July 1890.

The Gateway of Lincoln's Inn.—The following letter has been forwarded to the Benchers of Lincoln's Inn, on behalf of the Society of Antiquaries, by the president of the society:—"Society of Antiquaries of London, Burlington House, Piccadilly, W., July 2, 1890. To the Benchers of the Honourable Society of Lincoln's Inn. My Lords and Gentlemen,—In December, 1885, at a meeting of the Society of Antiquaries of London, I was authorised by a unanimous vote of the society to sign a memorial praying you to countermand the threatened destruction of the gatehouse court of Lincoln's Inn with the chapel, hall, and the interesting chambers known as No. 24, where once lived Secretary Thurlow. Since that time the buildings have been preserved, but it now appears that they are again in danger of destruction. From an independent survey of competent architects, I have reason to believe that the dilapidated condition of the buildings has been much exaggerated, and that by a comparatively small expenditure on judicious repairs the buildings may be placed in such a condition that they will last for many generations to come. I therefore again venture to address you in the name of this society, and to intercede on their behalf for the preservation of these interesting memorials of a past age, the destruction of which would be as deeply regretted as it is apparently needless. I have the honour to be, my Lords and Gentlemen, your obedient servant, John Evans, President of the Society of Antiquaries of London."

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LINCOLN'S INN GATEWAY.

Sir,—As you are going to give us some more "exactly-described" knowledge about this "interesting" ruin, and profess your readiness to give it on both sides, I send a few more remarks on your four manifestoes of July.

In spite of a warning last month which would have made any man pause who is not perfectly reckless, you plunge on to back up your statement that "rebuilding could not for a moment be defended from an investment point of view," by making some financial assertions even more demonstrably wrong than that was, and nothing to the purpose if they were right. By some inferential conjuring that I am not concerned with, you profess to prove that our rebuilding of chambers hitherto has produced "an actual loss of income." The sentence is rather obscure, but if it does not mean people to understand that, it means nothing; and I know that it has made people so understand. Your suggestion that I ought to compare our new rents with what we might have got by turning the Chancery Lane front into shops is a nice specimen of what antiquaries will turn round to say the moment they want a pretext for vilifying those who will not listen to their nonsense. Now for the facts.

I happen to have (not made for this purpose, but when I was objecting to further rebuilding a few years ago, because we should have had to borrow money for it) a return of the results of all our rebuildings of chambers up to that time, showing the cost of every successive block, including all temporary loss of rent and interest, and the cost of all single chambers that we had to buy, which increased the cost of one block in a high proportion. Yet not even in that case is your statement true; and on the average of the very large sum that we have spent, the "profit rental" in that return was close on $3\frac{1}{4}$ per cent. From various causes some chambers, both old and new, are vacant, and, as you have such a passion for old ones, it is strange that you prefer to live in the new ones, and quite out of sight of your beloved Old Square and the abode of Thurloe. "Comment is superfluous," as newspapers say, on such audacious inventions about a plain matter of figures as you have chosen to publish; obviously for mere personal prejudice, and when you might easily have asked beforehand whether such statements would be safely true, as you have asked many other questions, I know.

For the same purpose (for it has nothing upon earth to do with this question) you choose to talk of my "unnecessary additions to Inigo Jones's chapel." I know that you avow your indifference to architecture and churches, except so far as they are food for antiquaries. But other people do not, and there was not a word of objection by anybody to our restoring the chapel as we did, which was all but falling, especially the roof, and had no vestry but a small closet with a candle, and no west wall but a mere party-wall which had been cut to pieces. It had not indeed the honour of harbouring a party of traitors and would-be murderers, like your pet No. 24, but only of having produced more archbishops, bishops, and other eminent dignitaries than any other church in England as its Preachers—far more than the Temple, with which of course we cannot compete in some ways, and have only spent about a seventh, I believe, of what that cost to restore.

After these specimens of your mode of controversy I shall notice no more of your assertions and insinuations, all made with the same object and motive, and only add a few remarks on your own and your professional experts' architectural engineering. I wonder that any man calling himself a lawyer did not see the absurdity of citing to a body of lawyers the opinions of a set of witnesses of that class, against those whom the Bench had consulted with exactly the same object as yours, but who confessed that they could not support it.

Are you and your architects really so ignorant as to suppose that large gas-pipes with corresponding nuts are not as good as smaller rods for tying such walls together? Or to imagine that our Clerk of the Works was such a fool as to think of screwing the leaning-out walls "into the perpendicular," which would have broken them to pieces in five minutes, unless a vast deal of other work was done to prepare for it? I am continually finding deeper depths than I had fathomed before in architectural engineering; and here is another. I suspect the pit is bottomless. They do, however, leave you the monopoly of folly, in mistaking the outside wooden slabs, which only spread the hold of the ties over the wall, for props, as mere pretences of danger.

But they have overshot their mark, and yours, by their elaborate descriptions of the mischief done to the building by "many and careless cuttings" by successive owners, and express their surprise at the building having stood at all. Their "reports" are full of "cracks," in one case "from top to bottom," "splits," "ugly fissures," "bulges," bad modern lead flats, with the timbers under them; parapets which must be rebuilt, and at least one chimney, and an indefinite quantity of "face brickwork," and part of the south tower (which by the way is not a tower at all, but only two fronts without an intervening wall); and they point out that the south wall of the north tower above the ground floor stands on nothing but a single prop; and the removal of the old wall there to make the footway has caused "the most serious rents of all" above; and the windows have been mostly widened to put in the mean sashes and panes which we see, and you admire, and which "are generally poor, or worn out," and "the usual plank lintels" over them, "and other poor building contrivances without proper consideration," which, of course, have shrunk and bent, and caused more cracks above, "due to want of care in the insertion of the windows"; and "a cellar has been dug out much deeper than the foundations, and close to them; and here there is a slight settlement visible in Chancery Lane," though you "have verified the levels, and satisfied yourself that the footings have not sunk an *inch*"; another brilliant specimen of your ideas of an insignificant sinkage. And "other parts of the building require to be taken out and rebuilt," and "the whole *attended* to from the foundations to the chimneys." I do not remark on the architects' omissions, but only on their admissions.

When men of common sense, at all versed in the ways of "experts," find three reports full of such statements as these against the party that employed them, they can thoroughly appreciate those expert attempts to override their own admissions by a mere assertion that "there are no serious difficulties to overcome," and that "the work may be well done by a clerk of the works under the clear instructions of a practical surveyor," which means that an architect is to be turned in to do what he likes.

And all what for? Not even for the pretence that this place can ever be made habitable again according to modern wants, in such a sense that any gentleman will live or practise there; especially now that there are so much better chambers to be had, besides other old ones not quite so bad as these for those who, unlike you, have a taste for using them; or who being there, do not think it worth while to move. At any rate, you and your friends have been kind enough to prove that the place is a mere ugly ruin. I wonder how many of your screamers have

taken the trouble to go and look at it. I know that some of the most furious have not. So now I take leave of you.

Batch Wood, St. Albans. GRIMTHORPE.

P.S.—I just add that this was written and in type before you and your allies gave me the chance of answering you in the *Times* of July 21.

[I am glad to read Lord Grimthorpe's last sentence, for our readers would probably feel that they were having too much of Lincoln's Inn if the discussion were continued.

I will therefore be careful not to write anything that could give Lord Grimthorpe the right of claiming a further hearing on this subject. To carry on the discussion would be useless, from my point of view, for the Benchers have now decided not to pull the gateway down; and Lord Grimthorpe cannot assert that he is silenced, for he has opened the question in the *Times*, and even *Mr. Punch* has joined in recommending his "Dear Noble correspondent to the *Times*" to let Lincoln's Inn Gateway and archæology alone.

I will therefore content myself with saying that, as far as I have data to go upon, the figures do not seem to me to support Lord Grimthorpe's computation as to interest. I have not been able to learn the total sum expended by the Benchers in rebuilding, or the total rentals (old and new), though (encouraged by Lord Grimthorpe's statement that I might have asked) I have made application to the steward of the Inn for the figures; but was, as I expected, courteously told that he was not at liberty to give me such information. This secrecy with regard to the affairs of the Inn is, I think, rightly regarded as indefensible by the members of the Bar, who are aware that in the time of the Commonwealth members of Lincoln's Inn received a life interest in their chambers on paying a fine of twenty-five shillings to the Bench. The amount of the fine soon rose to ten pounds, and then to twenty; in George the First's time it was generally about a hundred pounds, and it is only during the present century that members of the Bar have been reduced to the position of ordinary tenants at an annual rental, without any voice in the expenditure of the large revenue derived from them.

Lord Grimthorpe was good enough to divulge the secret of the Benchers with regard to the cost of building his last block of chambers, and we are able to make an approximate estimate as to the old and new rentals. I see no reason to alter the statement I made in the last number on the subject. As to the cost of making Lord Grimthorpe's addition to Inigo Jones's chapel, I estimate the loss of revenue to the Inn to be about £2,000 a year; that is the interest on £10,000 expended in building, and £1,500 a year the rental of chambers pulled down to enable Lord Grimthorpe to alter the proportions of Inigo Jones's chapel and improve a vestry with which, according to his own showing, "archbishops, bishops, and other eminent dignitaries" had been content. The chapel is but little used now-a-days, and there is certainly no general wish amongst the Bar of Lincoln's Inn to compete with the Sunday shows at the Temple.

Lord Grimthorpe's great energy finds vent in building; he enjoys the construction and evidently also the destruction it involves, as some men enjoy sport. He is willing to pay highly for the right to exercise his hobby at St. Alban's Cathedral, and Lincoln's Inn was a sort of free warren for him. He is naturally annoyed, as sportsmen always are when their sporting rights and liberties are interfered with. I regret that I should have felt that it was necessary in the public interest to interfere with what contributes to his enjoyment; for, putting aside some antiquarian and other minor matters, there are many more important questions on which I heartily agree with him. I sincerely respect him for his attack on the system of paying commissions to agents, and for his out-spoken opposition to the endowment of research, and the awarding of medals by members of scientific societies to their contemporaries and co-workers.—A. C. RANYARD.]

Prof. H. M. PAUL, of the U.S. Naval Observatory, has discovered a remarkable new variable star (S. Antliæ), which appears to be of the Algol type, with a period of 7h. 46m. 48s., the shortest yet discovered. It varies about six-tenths of a magnitude from 6·6 mag. to 7·2 mag. And according to Mr. Chandler it remains at maximum brightness about 4h. 30m., the decrease and increase each occupying 1h. 40m. In other words, the eclipsing star only occupies about 35 minutes in passing from extreme elongation to the position where it commences to cut down the light of its primary—a time which seems to indicate that the dark body is within a radius of the bright star and far within Roche's limit ($2\frac{11}{12}$ radius), within which a satellite cannot exist, revolving round a homogeneous primary. Unfortunately this star is situated too far south for English observers at R.A. 9h. 26m. 50s. S. Dec. 28° 4′ 43″.—A. C. R.

TEETH AND THEIR VARIATIONS.

By R. LYDEKKER, B.A.Cantab.

(*Continued from page* 178.)

OUR last illustration of the gradual increase in the complexity of the structure of the grinding teeth in the Ungulate order, as we proceed from the old extinct generalised types (and some allied ones which still survive) to the specialised forms characteristic of the world of to-day, will be derived from the peculiar group of Elephants. At the present time, it need scarcely be mentioned, there are but two species of Elephant, both nearly related, but one being confined to the African Continent and the other inhabiting India and some of the adjacent regions. In the latter Tertiary period of the earth's history Elephants were, however, much more numerous, and were spread over the greater part of the surface of the globe, having been obtained from Europe and Asia, as far north as Siberia, North and South America, and North Africa. Many of these extinct Elephants, and, indeed, all of the earlier ones, differed very remarkably from the living species in the much simpler structure of their teeth; these species being known as Mastodons, a term which has now become almost a popular one.

FIG. 12.—AN ANTERIOR CHEEK-TOOTH OF A MASTODON.

In common with true Elephants, Mastodons differ from other Mammals, in that, instead of having all their cheek-teeth in use at the same time, the hinder ones gradually come up in an arc of a circle behind the tooth in use at any one particular period, which is gradually worn away and shed. Further, the teeth gradually increase in complexity from before backwards, the most anterior ones in some cases not having more than two ridges (Fig. 12), while the hinder ones are much more complex (Fig. 14). It results from this peculiar mode of succession that there are never more than portions of two, or at most of three, teeth on either side of each jaw in use at any one time. Fig. 12 shows that the simple cheek-teeth of a Mastodon are really constructed on the same general plan as those of a Pig (Fig. 4), the outer cones having more or less completely united with the inner ones to form two transverse ridges. In some of the earlier teeth of the Mastodon there may be only two such ridges (Fig. 12), but in the later

ones the number of ridges is generally either three or four (Fig. 13), with additional imperfect ridges at the two extremities. In such a tooth (Fig. 13) it will be seen that the transverse ridges are low and roof-like; the valleys separating one ridge from another being broad and comparatively shallow, without any of the substance known as cement at their bases. When the enamel on the summit of such ridges is worn through by the abrasion of the teeth of the upper jaw against those of the lower, oval or trefoil-shaped islands of the underlying dentine are revealed, as in Fig. 14. The latter figure exhibits the last tooth of a Mastodon in which there are five complete transverse ridges, this being the most complex form of tooth found in any of the Mastodons.

In certain Tertiary rocks in India lying along the

Fig. 13.—A Left Upper Cheek-Tooth of a Mastodon in an Unworn Condition.

southern flanks of the great range of the Himalaya there are, however, found enormous numbers of teeth of peculiar species of Elephants, which, while resembling in many respects those of the Mastodons, have a considerably greater number of ridges. Moreover, the ridges themselves are relatively narrower and taller, so that the depth of the intervening valleys is likewise increased. These valleys also contain a certain amount of the third constituent of mammalian teeth—the cement—so that their bases are partially filled up by it.

From the teeth of these Intermediate Elephants, as they may be conveniently called, the step is very short to those of the modern or true Elephants. In these, as shown in Fig. 15, the difference from the teeth of the Mastodon is so great that without the intermediate forms it is difficult to trace the correspondence between their respective elements. In the true Elephants, indeed, the transverse ridges of the teeth have not only been greatly increased in number, so that there may be as many as twenty-five in the last tooth of certain species, but they have also been so much increased in height and narrowed in width that they assume the form of thin plates, which may be six or eight inches in height, and of which the sides are almost parallel. The valleys between these plates, as they may now be called, have likewise become thin and deep slits, which are completely filled to their very summits with the cement. Thus, comparing Fig. 15 with Fig. 14, it will be apparent that each of the elongated discs seen in the former, which consist of a layer of enamel surrounding a strip of dentine, correspond to the transverse ridges of the latter; while the space between the discs in Fig. 15, which are composed of cement, represent the open valleys of Fig. 14.

The surface of such an Elephant's tooth forms, indeed, a millstone most perfectly adapted for grinding vegetable substances, consisting as it does of parallel ridges composed of elements of different degrees of hardness. Such a tooth, with its height of nearly eight inches in some species, takes many years to wear away; and with a succession of six of these teeth gradually increasing in size and complexity from the first to the last, we are well able to understand how the Indian Elephant can live fully to the age of a century. It is also equally evident that the more simple and lower-crowned teeth of the Tertiary Mastodons must have been worn away at a far more rapid rate; so that we are justified in saying that these animals could not have attained anything like the length of life enjoyed by their modern descendants.

There is a considerable amount of variation in the structure of the grinding teeth of the true Elephants, although all of them resemble to a greater or less degree the specimen represented in Fig. 15. In the African elephant, however, the discs of dentine, surrounded by their border of enamel, are much wider in the middle than in the figured tooth, and thus assume a lozenge shape. In this respect, therefore, the African elephant is a more generalised or old-fashioned kind of animal than his Indian cousin; and we may observe, in passing, that the African continent is now remarkable for containing a number of old types, such as Hippopotamuses, Giraffes, and Aard-Varks, which have totally disappeared from other regions, although, as we know from their fossil remains, they were once widely spread over the globe. Thus, Hippopotamuses (or shall we say Hippopotami?) once ranged over the greater part of Europe, extending as far north as the southern parts of our own islands, and were also common in northern India; the same being true of

Fig. 14.—The Last Left Upper Tooth of a Mastodon, with the enamel of the two first ridges perforated by wear.

the Giraffes, with the exception that their remains have not hitherto been found in Britain.

The most complex type of teeth is, however, attained by the Indian Elephant, and the closely allied Mammoth, which in the latest geological epoch ranged over the greater part of Europe, and whose frozen carcases are from time to time washed out from the so-called "tundras," or superficial deposits of Siberia, to be exposed to human view after having been buried for countless centuries. In these two species the plates of the teeth are narrower and

XUM

XUM

COWEN & Co
CEYLON.
ARTOCARPUS INCISA. (BREAD-FRUIT). 1133.

XUM

more numerous than in the tooth represented in Fig. 15, so that the even surfaces of dentine form still narrower strips. At first sight it seems difficult to believe that the Elephant now inhabiting the burning plains of India should be closely allied to a species which formerly roamed over the icy regions of Siberia, but there are two considerations which show how little value such objections have. Thus, in the first place, there is considerable evidence that the climate of Siberia, although doubtless always cold in winter, was formerly less severe than at present. We have, moreover, evidence in the case of the Tiger how an animal can support the extremes of heat and cold with no alteration of its structure. Many people, indeed, if they were asked to mention the *habitat* of the Tiger, would say India, little knowing that this creature ranges in China and thence to Siberia into extremely high latitudes. The skins of these northern tigers are far handsomer than those from India, the hair being long and comparatively shaggy, so as to protect its owner from the bitter cold. The case of the Indian Elephant and the Mammoth is a precisely similar one, the existing Indian species having, as we all know, an almost naked skin,

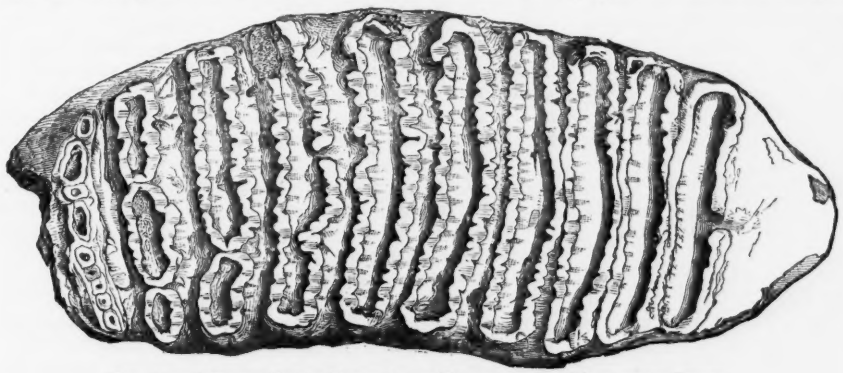

Fig. 15.—A Right Upper Tooth of an Elephant, in a Half-worn State.

while the Siberian Mammoth was clothed with long shaggy hair, as we learn not only from its frozen remains, but also from the rude pictures of the living animal drawn on fragments of its own tusks by the old pre-historic hunters of the Dordogne many centuries ago.

In mentioning tusks, we should not forget that the modern Elephants differ from many of the old Mastodons in having tusks only in the upper jaw, while in the latter they were also present in the lower. We have, therefore, here also another instance of the greater specialisation of the recent types.

In showing that with the advance of time the Elephants have gradually developed an extremely complex type of grinding tooth from a comparatively simple one, we find that they occupy a parallel position with that held by the other two great groups of the Ungulate order.

There is, however, one very important point whereby the Elephants differ from these two groups, namely, that they have not undergone any contemporaneous modification in the structure of the foot. The foot of an Elephant is, indeed, of an exceedingly primitive type, having five complete toes, and being more like those of the very earliest Ungulates of the Tertiary period than is the case with any other living member of the order, except the little Hyrax or so-called Coney of the Bible. The explanation of this absence of modification in the structure of the foot of the recent as compared with the extinct Elephants is, however, not far to seek. By reason of their huge bulk these animals have no need to fear the attacks of other creatures, so that there was no necessity for the development of a limb suitable for extreme speed, while the huge bulk of their bodies of itself also required the retention of a type of limb constructed on principles of strength rather than elegance. Hence there has been no inducement for any alteration in the structure of the feet of these ponderous brutes as time rolled on; and so that while it requires the aid of the man of science to trace the relationship existing between the tooth of the ancient Mastodon and that of the modern Indian Elephant, the veriest tyro would not hesitate to declare that the skeleton of the same two creatures indicated the closest relationship.

To sum up the results of this brief survey of some of the more striking features connected with the cheek-teeth of the Ungulate Mammals, we may say that all the Ungulates of the lower Tertiary deposits had low-crowned teeth of comparatively simple structure; and that those of all the three groups into which the order is divided can be derived from a type of tooth not far removed from that possessed by the ancestors of the Pigs, the latter type being itself a modification from the still more primitive triangular type. In all the three groups of Ungulates, the crowns of the teeth have tended to increase in height and in complexity of structure with the advance of time; those Ungulates with the tallest crowned teeth being characteristic of the most recent period of the earth's history. Finally, that while in the groups containing the Ruminants and the Horses the structure of the feet has been modified *pari passu* with that of the teeth, to produce limbs capable of carrying their owners at an extremely rapid rate, in the Elephants, where there is no need for swiftness of flight, the feet have not undergone this adaptive modification, so that the whole structural alterations have been confined to the teeth.

In a subsequent communication we shall hope to illustrate some of the modifications of tooth-structure found in other groups of Mammals; but we venture to think that sufficient has been said to show what an extremely interesting study is that of teeth. These organs are, indeed, interesting not only from their curious and often beautiful forms, but also, as we have striven to show in the foregoing paragraphs, as affording a concise and, so to speak, epitomised summary of the degree of evolution which any particular group of animals has undergone in the course of time.

WEISMANN'S THEORY OF HEREDITY.

By Edward Clodd.

THE scientific world is becoming interested to the point of agitation about certain theories which have been broached by a very distinguished German biologist, Professor August Weismann,* of Freiburg. These theories have been set forth and defended in a series of essays, issued at intervals

* *Essays upon Heredity and Kindred Biological Problems.* By Dr. August Weismann. Authorised Translation, Edited by E. B. Poulton, Selmar Schönland, and Arthur E. Shipley. (Oxford: Clarendon Press, 1889.)

since 1881, abstracts of which by Professor Moseley and others have appeared from time to time in *Nature*, and, under the title of the "Transmission of Acquired Characters," were the subject of an important debate opened by Professor Ray Lankester at the Manchester meeting of the British Association in 1887, Professor Weismann taking part in the discussion. Within the last few months the whole of Professor Weismann's essays have been collected and issued by the Clarendon Press as one of the volumes of the series of Foreign Biological Memoirs, so that the English reader has now the opportunity of learning about the last deliverances on the subject of "heredity and kindred problems" in his mother tongue. The publication of this volume gave rise to an able and, in fundamental points, adverse criticism by Sir William Turner at the Newcastle meeting of the British Association last autumn, and this has been followed by the appearance of a translation of Professor Eimer's book* on the same lines, as well as by Professor Vines's attack in *Nature*, to which Professor Weismann has replied in that journal, and by a voluminous correspondence which shows no sign of yet coming to an end. So that a considerable body of literature of the subject exists, and as the substance of what Professor Weismann has said is in scattered form not very easy to follow through the detached essays, an attempt to focus and present it in untechnical form may be of service. The learned Professor had asked himself, as every thoughtful biologist pondering over the mystery has asked: "How is it that a single microscopic cell, imbedded in the ovum of the mother, can reproduce the physical and mental features of the parents down to the minutest details, and with these, not infrequently, certain characteristics of grand-parents and of more remote ancestors?"

Several converging lines of observation and thought led him to the theory of the basis of heredity which is given in the essays, and of these the two following are the main tracks. Firstly, that death is not a primary attribute of living matter. Secondly, that characters *acquired* by the parent are *not* transmitted to the offspring.

Professor Weismann admits that the problem of the origin of life remains unsolved, and probably is insoluble; we know that life had a beginning and that it will have an end, but we know nothing more about it. The case is different, however, in respect to death. There is a vast number of living things which do not die naturally. Of course they perish through various causes; they are eaten, or destroyed by accident, by intense heat and other agencies. But so long as the normal conditions which brought them into being remain; so long as the conditions necessary to their existence are fulfilled; they neither decay nor die, having within them the power of an endless life. The organisms upon whom is thus laid no burden of mortality, with such pangs of separation, and such dread of the unknown, as are ours, lie at the very bottom of the scale of life. All livings things are, for purposes of convenience, divided into two classes: the Protozoa or one-celled, and the Metazoa or many-celled. The one-celled class includes the lowest and simplest forms, and the many-celled includes all other organisms in unbroken continuity of development, as, in animals, from sponges up to men.

Now it is the Protozoa which Professor Weismann says are alone immortal. Each Protozoon is a microscopic mass of a jelly-like substance, seemingly structureless, without apparent unlikeness or separation of parts. There is in the very simplest no body-cavity, no trace of a nervous system, but only a nucleus or minute particle near the centre. Every part of this one-celled organism does everything. Food and air are absorbed through the general surface; and locomotion through the water, in which medium it lives, is effected by pushing out finger-like processes. Although under certain conditions, as of drought or frost, it dries up, it resumes an active life when favourable conditions return. It performs an arithmetical feat in multiplying itself by division. The more it eats the bigger it gets, till such disproportion between the mass which needs food and the surface through which the food is received is reached that it divides equally, at the nucleus, into two parts. Each half becomes a complete individual, and grows in like manner till it also divides; and so on with the multiplication of Protozoa *ad infinitum*. Now it cannot be said of either half that one is parent and the other offspring, for both are of the same age, and only in a limited sense can we speak of succession of generations as the sub-divisions into separate individuals are repeated. Nor is there anything analogous to death in these processes. "There are," Professor Weismann says, "no grounds for the assumption that the two halves of an amœba are differently constituted internally, so that, after a time, one of them will die while the other continues to live. Such an idea is disproved by a recently discovered fact. It has been noticed in one of the foraminifera, and in other animals of the same group, that when division is almost complete, and the two halves are connected only by a short strand, the protoplasm of both parts begins to circulate, and for some time passes backwards and forwards between the two halves. A complete mingling of the whole substance of the animal, and a resulting identity in the constitution of each half, is thus brought about before the final separation."* Of course, as Professor Weismann points out elsewhere,† the immortality of one-celled organisms and (as will be shown presently) of the germ-cells of the Metazoa, is potential, not absolute. "It is not that they *must* live for ever, as did the gods of the ancient Greeks—Ares received a 'mortal wound' and roared for pain like to ten thousand bulls, but could not die—they can die, the greater number in fact do die, but a proportion lives on which is of one and the same substance with the others." An immortal, unalterable living substance does not exist, but only immortal forms of activity of organised matter.

Now, as the Metazoa are descended from the Protozoa, one is curious to learn how Professor Weismann explains the evolution of the mortal from the immortal, because, as we shall see, the many-celled have "put on mortality." He accounts for this by "unequal fission," in other words, the failure of certain one-celled organisms to divide themselves completely, whereby unlikeness of parts and differences of position of parts resulted. "The first multicellular organism was probably a cluster of similar cells, but these units soon lost the original homogeneity. As the result of mere relative position there arose division of labour, some of the cells were especially fitted to provide for the nutrition of the colony, while others undertook the work of reproduction."‡ Obviously, those on the outside, being exposed to the direct and constant action of their surroundings, would be the media of nutrition, and be builders-up of the cell-commonwealth. So the result of

* *Organic Evolution as the Result of the Inheritance of Acquired Characters according to the Laws of Organic Growth.* By Dr. Theodor Eimer. Translated by J. T. Cunningham, M.A. (Macmillan & Co., 1890.)

* *Essays on Heredity, &c.*, p. 26.
† *Nature*, 6 Feb. 1890, p. 318.
‡ *Essays*, p. 27.

this cell-clustering would be that the cells fell into two classes, body-cells and germ-cells. While the former were concerned solely with the nutrition of the organism, losing in this specialisation of function the power of reproduction, that function became concentrated in the germ-cells, or, speaking more precisely, in the germ-plasm, "the undying part of the organism," which is located in the nucleus of the germ-cell. It is these germ-cells which Professor Weismann contends are the immortal part of the Metozoa. "It is necessary," he says, "to distinguish between the mortal and the immortal part of the individual —the body in its narrow sense (the *soma*, as Professor Weismann, applying the Greek word for body, calls it) and the germ-cells. Death affects only the former; the germ-cells are potentially immortal, in so far as they are able, under favourable circumstances, to develop into a new individual, or, in other words, to surround themselves with a new body (*soma*)."* With increasing sub-division of function, there has been increasing modification of the organism, increasing complexity of parts, but the twofold classification of the cells has remained. The death of the body-cells is involved in the ultimate failure to repair waste, because a worn-out tissue cannot for ever renew itself, and because cell-division has its limits. Death also becomes a necessity, being of advantage to the species, the needs of which likewise determine the duration of the individual life.

Now as it is impossible for the germ-cell to be, as it were, an extract of the whole body, and for all the cells of the body to despatch small particles to the germ-cells from which these derive their power of heredity—the fundamental idea, it will be remembered, in Darwin's provisional theory of Pangenesis, namely, that all the cells throw off gemmules, which ultimately become concentrated in the reproductive elements—the germ-cells, so far as their essential and characteristic substance is concerned, are not derived from the body of the individual, but directly from the parent germ-cell. Heredity, then, according to Professor Weismann, is secured by the transference from one generation to another of a substance with a definite chemical and, above all, molecular constitution, and he names this theory "The Continuity of the Germ-Plasm." This germ-plasm is assumed to possess a highly complex but extremely stable structure, conferring upon it the power of developing into a complex organism. So stable is it, that "it absorbs nourishment and grows enormously without the least change in its complex molecular structure."† Of this germ-plasm it is assumed (we find no lack of assumption) that a small portion contained in the parent egg-shell is not used up in the construction of the body of the offspring, but is reserved unchanged for the formation of the germ-cells of the following generations. "One might represent the germ-plasm by the metaphor of a long creeping root-stock from which plants arise at intervals, these latter representing the individuals of successive generations."‡

Only variations of the germ-plasm itself are inherited, and it is upon these variations that natural selection operates. (Be it always remembered that, in Darwin's own words, "unless profitable variations occur, natural selection can do nothing.") Variations are due, Professor Weismann says, to the process of reproduction by which the larger number of existing organisms are propagated. This process combines two groups of hereditary tendencies derived from the mingled germ-plasms of the male and female parents, resulting in those individual differences which form the material from which new species are produced by the action of natural selection. Those differences multiply in geometrical ratio, so that "in the tenth generation a single germ contains 1,024 different germ-plasms with their inherent hereditary tendencies, and, as continued sexual reproduction can never lead to the reappearance of exactly the same combinations, new ones must always arise."* Sexual reproduction could alone "have called into existence that multiplicity of form of the higher animals and plants, and that constantly fluctuating union of individual variations, of which natural selection stood in need for the creation of new species."†

Such, in as brief outline as lies in our power to make, is the remarkable theory which is causing no light searchings of heart among the Bereans of Evolution, and renewed study of their scriptures to see whether these things are so. The most staggering blow which Professor Weismann has dealt against current beliefs is in the denial of the transmission of individually-acquired characters which is involved in his assumption of the continuity of the germ-plasm. Certainly, if in this matter he has not proved his case, he has exposed the insufficiency of the existing evidence against it. For what he says is that the structure of the offspring depends on the germ-plasm, and as this has no break in its continuity, but remains unaffected by any changes occurring within the body-cells, the structure remains identical. It matters not what may be the action of external influences, or of the use or disuse of certain organs; any changes induced thereby in the parent are not transmitted to the offspring. The parent is only the medium by which the germ-plasm repeats in the offspring the physical and mental structure of the ancestors, so that, as Grant Allen aptly puts it in his review of Professor Weismann's Essays,‡ "parent and offspring resemble one another, not because the parent produces the offspring, but because both arise from the self-same substance, which merely develops earlier in the parent and later in the offspring. To use a transparent metaphor, the father is thus reduced to the position of an elder brother to his own son." Professor Weismann restricts the term "acquired characters" to those features which make their first appearance in the individual, and which are due to mode of life different from that of its ancestors, to change of climate, variety of food, and to other agencies. To these may be added effects of mutilation. All such he classes as "somatogenic," because they follow from the reaction of the *soma* under external influences. It is these which he contends are not transmitted.

All other characters are classed as "blastogenic," because "they include all those characters in the body which have arisen from changes in the germ, and all the changes produced by natural selection operating upon variations in the germ."§

So the sum of the matter is, that natural selection is all in all, and that use and disuse, and action of the environment, count for nothing, or, perhaps, a very little, for upon this we have "two voices" in the *Essays*. The influence of the agents to which, as Darwin grew older and widened his range of observation, he was disposed to give greater weight, and the large place of which in the production of specific characters is maintained by Herbert Spencer in his *Factors of Organic Evolution*, is denied by Professor Weismann.

Our remaining space must be given to a few of the

* *Essays*, p. 122.
† *Ibid.*, p. 271.
‡ *Ibid.*, p. 266.

* *Essays*, p 276.
† Professor Weismann, in *Nature*, 6 Feb. 1890, p. 322.
‡ *Academy*, 1 Feb. 1890.
§ *Essays*, p. 413.

leading objections which tell in favour of the older view.

1. As against Prof. Weismann's contention against the rise of new species except through the agency of sexual reproduction, there are the numerous cases of a sexual fungi which show no sign of extinction, one family, as Prof. Vines points out, "of the most varied form and habit, including hundreds of genera and species, in which, so far as minute and long-continued investigation has shown, there is not, and probably never has been, any trace of a sexual process."* And like evidence is supplied by certain wheel-animalcules.

2. So far as acquired muscular developments and mutilations are concerned, doubtless the evidence of their transmission is of the slenderest kind. Generations of dogs whose tails and ears have been docked have not produced puppies with corresponding mutilations; the children of one-eyed, one-armed, and one-legged parents are born with their full complement of limbs and eyes, nor is it proved that the offspring of blacksmiths and navvies appear with abnormally developed biceps. But although this may hold good of gross and non-vital parts, Professor Weismann has a more difficult task in contending that it also does not apply to the subtle processes which initiate changes in vital parts. Upon this it is far from easy to get at his real meaning, for his statements are "hedged" with qualifications.

In his essay on "The Significance of Sexual Reproduction in the Theory of Natural Selection" it is admitted that "the ultimate origin of hereditary individual differences lies in the direct action of external influences upon the organism."† "In what way," he asks,‡ "could the transformation of species be produced, if changes in the germ-plasm cannot be transmitted? And how could the germ-plasm be changed except by the operation of external influences, using the words in their widest sense?" But immediately after the first of these extracts, he says, "Hereditary variability cannot, however, arise in this way at every stage of organic development, as biologists have hitherto been inclined to believe. It can only arise in the lowest unicellular organisms, and when once individual difference had been attained by these, it necessarily passed over into the higher organisms when they first appeared." Turning back to the essay on "The Continuity of the Germ-Plasm," we read as follows:—"I am also far from asserting that the germ-plasm is absolutely unchangeable or totally uninfluenced by forces residing in the organism within which it is transformed into germ-cells. I am also compelled to admit that it is conceivable that organisms may exert a modifying influence upon their germ-cells, and even that such a process is, to a certain extent, inevitable. The nutrition and growth of the individual must exercise some influence upon its germ-cells, but in the first place this influence must be extremely slight, and in the second place it cannot act in the manner in which it is usually assumed that it takes place."§ The manner in which the influence acts is a matter of observation which, from the nature of the case, is beset with difficulty; that the influence is slight is of quite secondary importance, because, however small may be the effect, its repetition and accumulation through generations will give us all the proof we need of transmission of functionally-acquired characters. Really, after such admissions on the part of Professor Weismann, there seems little left to argue about, but for his arbitrary arrest of the action of external influences in organisms above the Protozoa.

That these are many-celled does not destroy the fact of their fundamental unity, or make other than incredible the theory that the germ-plasm can exist in the organs of reproduction unchanged by the variations which modify, more or less, the whole *soma*. How is the theory of an unaffected, insulated germ-plasm to be reconciled with the ceaseless manufacture, secretion, and expulsion of germ-cells which goes on through active life, the materials of which are derived from the materials which nourish the entire organism, the complexity of the cells of which may, for aught we know, be as subtle as those of the germ-cells? And, moreover, how can that theory be reconciled with the subtile influences of altered physical conditions, and especially of the nervous system on the reproductive system?

Besides this, there is a multitude of organisms in which the germ-plasm is not located in one place, but diffused throughout. Any part of a *Hydra* when cut off will grow into an entire animal; fresh plants, producing flowers and fruit, will grow from the fragments of the leaves of the *Begonia*, and Professor Eimer cites the case of a forest of young fronds which sprouted from a thallus of *Lunularia vulgaris* which had been "cut up with a sharp knife on a smooth plate of cork until the fragments were so small as to form a coarse-grained pulp,"* which was then spread on moist sand. Such instances as these, to which the familar case of the propagation of the potato through the tuber may be added, render it "difficult," as Sir W. Turner remarks, "to understand why the nutritive processes which affect and modify the soma-cells should not also react upon the germ-plasm."

3. Space forbids detailed reference to the striking experiments of Hoffmann on wild flowers, in which double flowers obtained by continuous cultivation from normal wild flowers became hereditary, and to the observations of Yung on change of sex of tadpoles by altering the nature and quality of their food,† and we must pass to what appears the chief *crux* in Prof. Weismann's theory, namely, the impossibility of reconciling psychological evolution with the continuity of the germ-plasm.

Among the most solid contributions which Mr. Spencer has made to biology in its highest aspects is his theory of the genesis of the nervous system, a theory which is confirmed by the observations of the lamented Francis Balfour and other embryologists. That system, both in man and the lower animals, had a common origin in modifications of the primitive skin due to the direct action of the environment. The irritability which characterizes the entire surface of the lowest animals gradually became concentrated in definite tracks and led to the formation of nerve-centres. "The functions of the central nervous system, which were originally taken by the whole skin, became located in a special part of the skin which was step by step removed from the surface,"‡ the brain itself "arising from an infolded tract of the outer skin which, sinking down beneath the surface, became imbedded in other tissues and eventually surrounded by a bony case."§ Here, if anywhere, seems incontrovertible proof of the origin of the structures through which we apprehend the outer world in the play of that outer world upon the superficial parts of the organism. "These structures," as Mr. Spencer says, "once commenced, and furthered by natural

* *Nature*, 24 Oct. 1889, p. 626.
† *Essays*, p. 279.
‡ *Ibid.*, p. 411.
§ *Ibid.*, p. 170.

* Eimer, p. 395.
† *Evolution of Sex*, by Geddes and Thomson, p. 41.
‡ Spencer's *Factors of Organic Evolution*, p. 66.
§ Balfour's *Treatise on Comparative Embryology*, vol. ii., p. 400 (Second Edition.)

selection where favourable to life, would form the first term of a series ending in developed sense-organs and a developed nervous system."

How serious are the issues involved is pointed out in Mr. Spencer's preface, as the following extracts show:—

If functionally-produced modifications are inheritable, then the mental associations habitually produced in individuals by experiences of the relations between actions and their consequences, pleasurable or painful, may, in the successions of individuals, generate innate tendencies to like or dislike such actions. But if not, the genesis of such tendencies is, as we shall see, not satisfactorily explicable.

That our sociological beliefs must also be profoundly affected by the conclusions we draw on this point, is obvious. If a nation is modified *en masse* by transmission of the effects produced on the natures of its members by those modes of daily activity which its institutions and circumstances involve, then we must infer that such institutions and circumstances mould its members far more rapidly and comprehensively than they can do if the sole cause of adaptation to them is the more frequent survival of individuals who happen to have varied in favourable ways.

I will add only that, considering the width and depth of the effects which acceptance of one or other of these hypotheses must have on our views of Life, Mind, Morals, and Politics, the question—Which of them is true? demands, beyond all other questions whatever, the attention of scientific men.

For this reason, Professor Weismann merits the thanks of his opponents in stimulating their inquiry into the soundness of the foundations on which their belief in the transmission of individually-acquired characters rests. Each has the fact of the continuity of generations to work upon; the rest is a question of evidence which has only recently been collected with any pretence to accuracy of record, or with any true appreciation of its profound significance. So that we have scarcely emerged from the empirical stage, and have long to wait before our material can be of sufficient volume to be of value.

Much remains to be added to give this paper any pretence to completeness of exposition, but we must supplement its shortcomings by commending to the careful perusal of our readers Sir William Turner's Address, already referred to, which is given in full in *Nature*, 26th September 1889, as also Mr. Cunningham's masterly preface to his translation of Professor Eimer's book for a series of arguments, drawn from his own researches, in support of variations through functional activity and external conditions.

THE FACE OF THE SKY FOR AUGUST.

By Herbert Sadler, F.R.A.S.

THE remarkable sunspot minimum still continues, the sun's disc for days together being entirely free from a trace of spots. Conveniently observable minima of Algol occur at 10h. 51m. P.M. on the 6th; 7h. 40m. P.M. on the 9th; and 9h. 22m. P.M. on the 29th. Mercury is an evening star, but is not suitably placed for observations by the amateur, as during the whole month he sets less than three-quarters of an hour after the sun. Venus is an evening star, setting on the 1st at 9h. 11m. P.M., 1h. 24m. after the sun, with a northern declination of 5° 2′ and an apparent diameter of $15\frac{1}{2}''$. On the 31st she sets at 7h. 22m., 0h. 34m. after the sun, with a southern declination of 10° 4′, and an apparent diameter of $19\frac{1}{2}''$. At the beginning of the month she appears as a little moon between twenty and twenty-one days old, and is then in Leo, but she enters Virgo on the 5th, and remains in that constellation till the end of the month. She does not approach any conspicuous star very closely. Mars is an evening star, though owing to his increasing southern declination and decreasing apparent diameter he will be found rather a disappointing object for the amateur. He sets on the 1st at 11h. 13m. P.M. with a southern declination of 23° 54′ and an apparent diameter of 14″. On the 31st he sets at 9h. 58m. P.M., with a southern declination of 25° 42′ and an apparent diameter of $11\frac{1}{4}''$. On the latter date his brightness is only one-third of what it was at opposition. About the middle of the month $\frac{86}{100}$ of the disc will be hidden from view. During August he passes through portions of Scorpio and Ophiuchus. At transit on the 5th he will be about 2′ due south of the $6\frac{3}{4}$ magnitude star Lacaille 6751; at 11h. P.M. on the 14th he will be about $7\frac{1}{2}'$ *n.p.* the $5\frac{1}{2}$ magnitude star 22 Scorpii; and at transit on the 30th he will be about $7\frac{1}{2}'$ due south of the $6\frac{3}{4}$ magnitude star 28 Ophiuchi. Jupiter is an evening star, rising on the 1st at 7h. 36m. P.M., with a southern declination of 19° 17′, and an apparent equatorial diameter of 48″. On the 31st he rises at 5h. 30m. P.M., with a southern declination of 20° 9′, and an apparent equatorial diameter of $46\frac{1}{2}''$. The following phenomena of the satellites occur while the planet is more than 8° above, and the sun 8° below, the horizon. An occultation disappearance of the third satellite at 8h. 55m. P.M. on 1st. An eclipse reappearance of the same satellite at 0h. 45m. 54s. on the 2nd; an occultation disappearance of the first satellite at 1h. 56m. A.M.; a transit ingress of the first satellite at 11h. 4m. P.M., and of its shadow at 11h. 9m. P.M. (see "Face of the Sky" for July). On the 3rd a transit egress of the first satellite at 1h. 24m. A.M., and of its shadow at 1h. 30m. A.M.; an eclipse reappearance of the first satellite at 10h. 46m. 44s. P.M. An occultation disappearance of the second satellite at 10h. 56m. P.M. on the 4th. An eclipse reappearance of the second satellite at 2h. 4m. 1s. A.M. on the 5th. An eclipse reappearance of the fourth satellite at 1h. 46m. 29s. A.M. on the 6th; a transit egress of the second satellite at 8h. 48m. P.M., and a transit egress of its shadow at 9h. 9m. P.M.; an occultation disappearance of the third satellite at 0h. 12m. A.M. on the 9th. A transit ingress of the first satellite at 0h. 48m. A.M. on the 10th, and of its shadow at 1h. 4m. A.M.; an occultation disappearance of the first satellite at 10h. 6m. P.M. An eclipse reappearance of the first satellite at 0h. 41m. 37s. A.M. on the 11th; a transit egress of the first satellite at 9h. 34m. P.M., and of its shadow at 9h. 53m. P.M.; an occultation disappearance of the second satellite at 1h. 10m. A.M. on the 12th; a transit ingress of the shadow of the second satellite at 8h. 52m. P.M.; a transit egress of the second satellite itself at 11h. 4m. P.M., and of its shadow at 11h. 47m. P.M. An occultation disappearance of the first satellite at 11h. 51m. P.M. on the 17th. A transit ingress of the first satellite at 8h. 58m. P.M. on the 18th, and of its shadow at 9h. 27m. P.M.; a transit egress of the same satellite at 11h. 18m. P.M., and of its shadow at 11h. 47m. P.M. A transit egress of the shadow of the third satellite at 8h. 45m. P.M. on the 19th; an eclipse reappearance of the first satellite at 9h. 5m. 19s. P.M.; a transit egress of the shadow of the third satellite at 10h. 47m. P.M. A transit ingress of the second satellite at 10h. 26m. P.M. on the 20th, and of its shadow at 11h. 29m. P.M. A transit egress of the second satellite at 1h. 21m. A.M. on the 21st. An eclipse reappearance of the second satellite at 8h. 33m. 28s. P.M. on the 22nd. A transit ingress of the first satellite at 10h. 43m. P.M. on the 25th, a transit ingress of its shadow at 11h. 22m. P.M. A transit egress of the first satellite at 1h. 4m. A.M. on the 26th; an occultation disappearance of the first satellite at 8h. 3m. P.M.; a transit ingress of the third satellite at 8h. 27m. P.M.; an eclipse reappearance of the first satellite at 11h. 0m. 24s. P.M.; a transit ingress of the shadow of the third satellite at

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11h. 8m. P.M. A transit egress of the third satellite at 0h. 7m. A.M. on the 27th; a transit egress of the shadow of the first satellite at 8h. 11m. P.M. A transit ingress of the second satellite at 0h. 44m. A.M. on the 28th. An eclipse reappearance of the second satellite at 11h. 9m. 41s. P.M. on the 29th. A transit egress of the fourth satellite at 11h. 8m. P.M. on the 30th. Jupiter describes a short retrograde path in Capricornus during the month, but does not approach any naked-eye star.

Saturn is in conjunction with the sun at 7h. P.M. on the 30th. Uranus and Neptune are, for the purposes of the amateur observer, invisible; the latter planet being in quadrature with the sun on the 30th. This month is one of the most favourable ones for observing shooting stars in. The most noted shower is that of the Perseids, with a radiant point at the maximum display on August 10 in R.A. 11h. 52m. Decl. + 56°. Observations of this region of the heavens with an opera-glass will, no doubt, show stationary meteors, or meteors which shift their positions very slowly. Their place and the direction of their shift should be noted for the purpose of determining whether the radiant is a geometrical point, or a circle, or an elliptic area, as suggested with regard to the November meteors (*Monthly Notices of the R.A.S.*, vol. xlvii. pp. 69-73). The radiant point souths at 5h. 37m. A.M. The moon enters her last quarter at 2h. 19m. P.M. on the 7th, is new at 4h. 20m. P.M. on the 15th; enters her first quarter at 1h. 20m. P.M. on the 23rd, and is full at 4h. 35m. A.M. on the 30th.

At 3h. 5m. A.M. on the 1st the $5\frac{1}{2}$ magnitude star χ Capricorni will disappear at an angle of 154° from the lunar vertex, and reappear at 4h. 4m. A.M. at an angle of 303. On the 4th the $4\frac{1}{2}$ magnitude star 30 Piscium will disappear at 1h. 55m. A.M. at an angle of 170° from the vertex, and reappear at 2h. 30m. A.M. at an angle of 234° from the vertex; while at 3h. 31m. the same morning the $4\frac{1}{2}$ magnitude star 33 Piscium will disappear at an angle of 186° from the vertex, and reappear at 4h. 40m. A.M., ten minutes after sunrise, at an angle of 309° from the vertex. At 11h. 34m. P.M. on the 9th the 5 magnitude ι Tauri will make a near approach to the lunar limb. At 1h. 9m. A.M. on the 10th the 6th magnitude star 105 Tauri will disappear at an angle of 98° from the vertex, and reappear at 1h. 58m. A.M. at an angle of 215° from the vertex. The $4\frac{1}{2}$ magnitude star 1 Geminorum will disappear at 2h. 48m. A.M. on the 11th at an angle of 31° from the vertex, and reappear at 3h. 40m. A.M. at an angle of 270°. The $6\frac{1}{2}$ magnitude star B.A.C. 2238 will disappear at 0h. 56m. A.M. on the 12th at an angle of 39° from the vertex, the star being below the horizon of Greenwich at the time, and reappear at 1h. 41m. A.M. at an angle of 268°. The $5\frac{1}{2}$ magnitude star ψ Sagittarii will disappear at 6h. 1m. P.M. (in bright sunlight) on the 26th, at an angle of 90° from the vertex, and reappear at 7h. 4m. P.M., five minutes after sunset, at an ample of 225° from the vertex. The $5\frac{1}{2}$ magnitude star 33 Capricorni will make a near approach to the lunar limb at 6h. 27m. P.M. on the 28th, in bright sunlight, and at 11h. 26m. P.M. the same evening the 6th magnitude star 37 Capricorni will disappear at an angle of 47° from the vertex, and reappear at 11h. 50m. P.M. at an angle of 11° from the vertex. At 0h. 35m. A.M. on the 29th the $4\frac{1}{2}$ magnitude star ϵ Capricorni will disappear at an angle of 149° from the vertex, and reappear at 1h. 38m. A.M. at an angle of 297° from the vertex; and at 3h. 18m. the same morning the 5th magnitude star κ Capricorni will disappear at an angle of 171° from the vertex, and reappear at 4h. 10m. A.M. at an angle of 306°, the star being below the horizon of Greenwich at the time.

Whist Column.

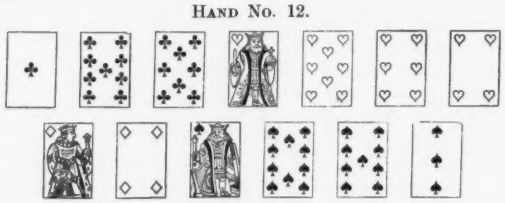
By W. MONTAGU GATTIE.

AMERICAN WHIST—THE "NEW PLAY."

IN noticing last month the new treatise by "G. W. P." on American Whist, we ventured to assert that the beginner who should endeavour to master the mysteries of the "new play," as therein set forth, would soon become hopelessly confused. Having now had time to make a more thorough examination of the book, and especially of the illustrative hands with which it concludes, we are reluctantly compelled to admit that we were wrong in restricting our remark to beginners. Entertaining, as he does not scruple to declare, a very poor opinion of English players, G. W. P. will probably not feel surprised at their failing to understand his precepts; but the fact is none the less unfortunate as affecting the value of his work in this country. From among a score of equally startling dogmas it will be sufficient to take a single example. On p. 99 we find this mandate: "Holding any two high cards in sequence and no more of the suit, upon partner's lead play the highest." We had imagined this to be the simplest and most orthodox method of indicating a desire for a trump lead. But G. W. P. goes on to say, "when the other falls, it is not a call. The play is informatory, and partner will judge as to his future lead." We have endeavoured to apply this maxim to a practical case. A holds four hearts to the ten and leads a small one; G. W. P., his partner, holding queen, knave, only, plays queen; fourth hand wins with king. Presently A, having the lead again, continues the suit; G. W. P. plays knave, and fourth hand wins with ace. How is A to divine whether G. W. P. has another heart and is calling for trumps, or whether the play is "informatory" that he has no more hearts? On what data is he to form a judgment as to his future lead? And, again, how would G. W. P. himself proceed, if he held queen, knave, and another heart, and wished to call? Or, to take a still more simple case, supposing him to play nine and then eight on his partner's lead of king and ace, would this be a call if he held the knave also, but only "informatory" if he did not? Either there must be this ambiguity, or G. W. P.'s maxim renders it nearly always impossible for third hand to call with two cards in sequence.

The following is given as a specimen of a game won by brilliant play, which, by the way, G. W. P. tells us is "very occasional" in England. We give the game (with the notes) as it stands, appending our reasons for thinking that it may also be described as a game lost by very bad play.

HAND NO. 12.

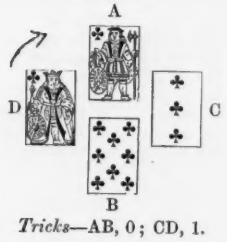


B's Hand.

Score.—AB, 6; CD, 4. [As explained last month, the American game is seven points up, and honours do not count. In this case honours are divided, and, if we place AB at 4, and CD at 2, we may consider the hand as having been played under the English code, the point being that CD require three by cards to make game.]

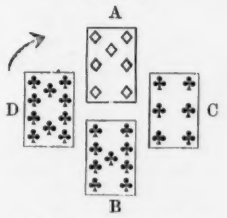
B turns up the eight of clubs.

TRICK 1.



Tricks—AB, 0; CD, 1.

TRICK 2.

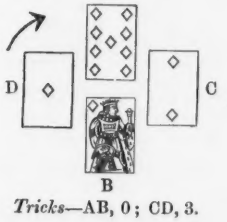


Tricks—AB, 0; CD, 2.

Trick 1.—D plays king at head of six.

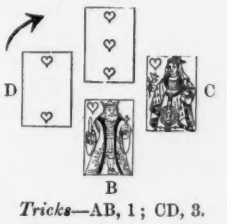
Trick 2.—C has not echoed, and does not hold both ace and seven.

TRICK 3.



Tricks—AB, 0; CD, 3.

TRICK 4.

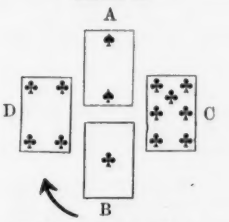


Tricks—AB, 1; CD, 3.

Trick 3.—The *coup de sacrifice*, a beautiful play.

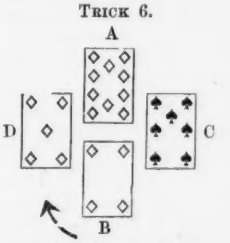
Trick 4.—The two of hearts, to throw the play, if possible, into C's hand, that on his lead D may get rid of his diamonds.

TRICK 5.



Tricks—AB, 2; CD, 3.

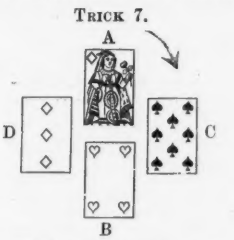
TRICK 6.



Tricks—AB, 3; CD, 3.

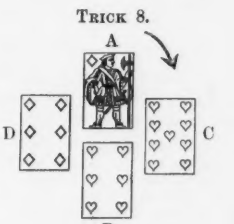
Trick 5.—B reads the six trumps of D, consequently the three of C's hand, also the five diamonds of D, and the best diamonds in A's hand.

TRICK 7.



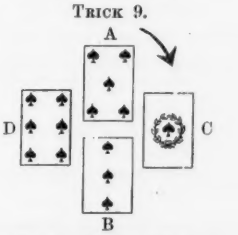
Tricks—AB, 4; CD, 3.

TRICK 8.



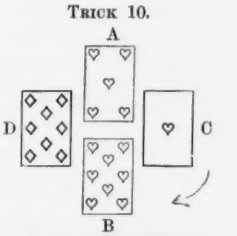
Tricks—AB, 5; CD, 3.

TRICK 9.



Tricks—AB, 5; CD, 4.

TRICK 10.



Tricks—AB, 5; CD, 5.

Trick 9.—C reads the rest of the trumps in D's hand, and but one spade and the last diamond, and plays ace.

TRICKS 11, 12, and 13 are taken by D's trumps,

CD MAKE TWO BY CARDS, AND
AB SAVE THE GAME.

A's Hand	B's Hand.
S.—5, 4, 2.	S.—Kg, 10, 9, 3.
H.—Kn, 7, 5, 3.	H.—Kg, 8, 6, 4.
C.—Kn.	C.—Ace, 9, 8.
D.—Qn, Kn, 10, 9, 7.	D.—Kg, 4.

C's Hand.	D's Hand.
S.—Ace, Qn, Kn, 8, 7.	S.—6.
H.—Ace, Qn, 10, 9.	H.—2.
C.—7, 6, 3.	C.—Kg, Qn, 10, 5, 4, 2.
D.—2.	D.—Ace, 8, 6, 5, 3.

REMARKS.—At Trick 3 many players in D's position would continue the trumps. This is obviously the best course if C holds the ace. If B has it, D has still to consider that he requires two tricks at least, if not three, from C in order to make game, and that he will be giving C's cards the best chance by throwing the lead into B's hand. If B has both ace and seven (the eight and nine may not improbably have been false cards), he is tolerably certain to make them sooner or later, and, as the game can only be won by strong cards in C's hand, it is, in this case also, desirable to place the lead with B. But, as D prefers to open the Diamonds, he is surely ill-advised in abandoning the command, especially as A has shown strength in the suit by his discard at Trick 2. The lead of ace from a five-suit is correct at the commencement of a hand, but not when the trumps are mostly out, and there is no doubt that the ace will make later on. D should have opened the diamonds with the five; B, pursuing the same tactics as in the actual game, would have won his partner's trick with the king, drawn a round of trumps, and returned the small diamond. D could then have played his ace, and continued with the three, showing five originally. A's best chance would have been to lead a heart or a spade; but C, if he had counted his partner's hand would have played his two aces, and D would eventually have made game with his trumps and the last diamond. B's play of the king is clever, but we do not see that there is any certainty that A holds all the best diamonds, as the foot-note to Trick 5 implies; he may have discarded from the best protected of three four-card suits.

D's play at Trick 4 is still more open to question. Why not continue the diamonds? The worst that can happen is for B to be void of them and to make the seven of trumps, if he has it; and in that event he will have to lead spades or hearts up to C. D's aim should be to clear his diamonds, not to seek means of throwing them away. C shows himself worthy of his partner. He only discovers at Trick 9 what is perfectly clear after Trick 3—viz. that D holds queen, five, four, two, of trumps, and at least four small diamonds, and therefore can have only two other cards. Consequently he embraces the opportunity which D affords him of throwing away the game by a *finesse*. His play is the more inexcusable inasmuch as the deuce of hearts is obviously a "singleton." The best of players may sometimes fail to count his partner's hand correctly, although the hands in which such lapses occur are not usually selected for publication in a standard work; but in this instance C's play appears to us to savour of dulness as well as inattention. Knowing that B could have no trump but the ace, it was clearly his duty to make sure of Trick 4, and to return the hearts at once for D to ruff. D

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might then have been nerved to continue the diamonds, and CD would have won the game easily and an extra point to boot, according to the American system of scoring. In fact, we scarcely see how CD could have played the hand worse than they did.

Chess Column.

BY I. GUNSBERG (MEPHISTO).

[Contributions of general interest to chess-players are invited. Mr. Gunsberg will be pleased to give his opinion on any matter submitted for his decision.]

THE following two interesting games were played in the match Blackburne *v.* Lee, at Bradford.

GAME No. 2.

WHITE. Lee.	BLACK. Blackburne.	WHITE. Lee.	BLACK. Blackburne.
1. P to K4	P to K4	23. Q to K3	QR to KBsq
2. Kt to KB3	Kt to QB3	24. P to KKt4 (*k*)	P to B3
3. B to B4	B to B4	25. P to R5	Q to B2
4. P to Q3	Kt to B3	26. K to B2	R to B5
5. B to KKt5	P to KR3	27. K to Kt2	Q to K2
6. B to K3	B to Kt3	28. R to Kt3	Q to Kt4
7. Kt to B3	P to Q3	29. R to KBsq (*l*)	P to QKt3
8. P to QR3 (*a*)	B to K3 (*b*)	30. Kt to Ktsq	P to B4
9. KB × B	P × B	(1h. 58 min.)	(1h. 59min.)
10. QKt to R4 (*c*)	Castles	31. Kt to B3	P to Kt5 (*m*)
11. Kt × B	RP × Kt	32. P × P	Kt to B7
12. Kt to Q2 (*d*)	Q to Ksq	33. Q to Bsq	Kt × P
13. P to QB3	Q to Kt3	34. Kt to K2	R(B5) to B2 (*n*)
14. P to KKt3	Kt to Kt5	35. Q × Q	P × Q
15. Q to K2	R to B3	36. R to Qsq	R to Q2
(58 min.)	(50 min.)	37. Kt to Bsq	P to B5
16. P to B3	Kt × B	38. K to B2	R(Bsq) to Qsq
17. Q × Kt	Q to R4 (*e*)	39. KR to Ktsq	Kt × P (ch)
18. P to KR4 (*f*)	Q to Kt3	40. Kt × Kt	R × Kt
19. R to R3	P to Q4 (*g*)	41. R to Ktsq	R to KBsq
20. Q to K2	P to Kt4	42. R to Kt3	R to Q7 (ch)
21. R to Bsq (*h*)	P to Q5	43. K to Ksq	R to R7 (*o*)
22. P × P	Kt × P (*j*)	(2h. 47min.)	(2h. 52min.)

White resigns.

NOTES.

(*a*) The game, so far, has proceeded on ordinary lines, but P to QR3 hardly adds effectively to White's development.

(*b*) The right reply to White's inactive move.

(*c*) If Black now plays B × B, the Knight will remain badly placed on Rook's 4th, and a loss of time in any event.

(*d*) Another move of an inactive character, and all the time Black is developing his game.

(*e*) This does not seem to be a profitable move.

(*f*) A good introduction to an attack by means of P to KKt4, &c.

(*g*) White threatened to castle; Queen's rook followed by R to Kt square; Black advances just in time.

(*h*) It is difficult to suggest a better move.

(*j*) Black boldly runs the risk of remaining with his isolated pawns should it come to an end game.

(*k*) P to B4 would lead to very interesting complications in which Black could give up the Knight.

(*l*) Black threatens R × BP.

(*m*) An excellent move by which Black gains a substantial advantage.

(*n*) Safe and sound, but Kt × QP was very tempting, but less reliable.

(*o*) A neat ending, and in harmony with Blackburne's play throughout.

The following is the third game played in the Blackburne *v.* Lee match at Bradford.

FRENCH DEFENCE.

WHITE. Blackburne.	BLACK. Lee.	WHITE. Blackburne.	BLACK. Lee.
1. P to K4	P to K3	8 QKt to Q2	P to B3 (*a*)
2. P to Q4	P to Q4	9. R to Ksq	QKt to Q2
3. P × P	P × P	10. P to KR3	B to R4
4. KKt to B3	KKt to B3	11. P to KKt4 (*b*)	B to Kt3
5. B to Q3	B to Q3	12. B × B	RP × B
6. Castles	Castles	13. Kt to K5	Q to B2
7. B to KKt5	B to KKt5	14. Kt × Kt	Kt × Kt
15. Kt to Bsq	KR to Ksq	40. K to Kt2	R to K8
(54 min.)	(48 min.)	41. QR to K3	K to Kt2
16. Q to Q2 (*c*)	Kt to Bsq (*d*)	42. Kt to B4	R to QB8
17. B to K3 (*e*)	Kt to K3	43. R to K2	P to KKt4
18. Q to Q3	R to K2 (*f*)	44. Kt to Q3	R to Q8
19. B to Q2 (*g*)	QR to Ksq	45. P to B3	P to KKt5 (*m*)
20. P to KR4 (*h*)	Q to Kt3 (*j*)	(2h. 58min.)	(2h 57min.)
21. B to K3 (*k*)	Q × KtP	46. KR to K3	R to KRsq (*n*)
(1h. 46min.)	(1h. 15min.)	47. Kt to K5	R to R5
22. KR to Ktsq	Q to R6	48. R to Ksq	R × R
23. R to Kt3	Q to R4	49. R × R	R to R6
24. P to R5	P × P	50. Kt × BP	R × P
25. P × P	Q to B2	51. Kt to Kt4	R to B6 (*o*)
(1h. 55min.)	(1h. 25min.)	52. Kt × P	R × P (ch)
26. Q to B5	Q to Q2	53. K to Ktsq	R to B4
27. Q to R3 (*l*)	Kt to B5	54. R × Kt (*p*)	R × Kt
28. Q × Q	R × Q	55. R × P (ch)	K to B3
29. Kt to Kt3	P to KKt3	56. K to B2	R to R4
30. QR to Ktsq	P to QKt4	57. R to Kt2	R to R6
(1h. 59min.)	(1h. 48min.)	58. K to Ksq	P to R4
31. P × P	P × P	59. K to Q2	R to R6
32. K to Bsq	Kt to K3	60. K to Bsq	K to B4
33. Kt to K2	Kt to Kt2	(3h. 56min.)	(3h. 54min.)
34. R to B3	R to QB2	61. R to K2	K to B5
35. B to B4	Kt to B4	62. K to Kt2	R to Q6
36. B × B	Kt × B	63. R to K5	R × P
37. R to Qsq	Kt to K5	64. R × P	
38. R to B3	R to R2	drawn.	
39. QR to Q3	R to R8 (ch)	(4 hours.)	(4 hours.)

(*a*) Always a safe move in the opening.

(*b*) Blackburne is never afraid of making a risky move in the hope of creating a lively complication.

(*c*) Q to Q3 would have been better.

(*d*) Intending to make a useful counter-demonstration by Kt to K3 and Kt to B5.

(*e*) When making a rapid advance any counter-demonstration in force will compel a retreat, often with loss of time and position.

(*f*) Black wisely intends to double his rooks on the king's file before making any advance.

(*g*) White has lost two important moves, for he might have played 16. Q to Q3, followed by 17. B to Q2.

(*h*) Played for want of a better move.

(*j*) Black's tactical advantage begins to materialise.

(*k*) White had not much time to examine 21. B to B3, Kt to B5; 22. Q to Q2, R to K7; 23. R × R, R × R; 24. Q to Qsq, &c.

(*l*) Q to B3 would have, for the moment, avoided the exchange of queens.

(*m*) It will be seen that in this subtle struggle for the ending, the experience of White has told in his favour somewhat.

(*n*) Played with a view to a mate by R to KR8, but I think Kt × BP would have been a simple way of winning.

(*o*) Black has not handled his game to the best advantage. This move in particular is an oversight, which loses a valuable pawn, and deprives him of any chance of winning.

(*p*) White might have forced a draw at once by Kt to K7.

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